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Articles

Publishing in Professional Journals

The Stochastic Coefficients Approach to Econometric Modeling, Part III: Estimation, Stability Testing, and Prediction

Agricultural and Rural Data Paradigms

Dynamic Factor Demands Using Intertemporal Duality

Contemporaneous Correlation and Modeling Canada's Imports of U.S. Crops

Two Methods for Estimating Real Structural Change in Agriculture

Book Reviews

Economic Efficiency in Agricultural and Food Marketing

U.S. Agriculture in a Global Setting: An Agenda for the Future

Productivity and Value: The Political Economy of Measuring Progress

Agricultural and Rural Areas Approaching the Twenty-first Century

The Farm Financial Crisis: Socioeconomic Dimensions and Implications for Producers and Rural Areas

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In This Issue

Swamy, Conway, and LeBlanc return with the third and final article in their series on stochastic coefficients. Building on the critique of fixed coefficients in the first article, and the specification of stochastic coefficients models in the second, they now extend their stochastic coefficients arguments into the realm of forecasting. They examine the sources of forecast errors and, employing a stochastic coefficients model designed by Swamy and Tinsley, account for most of the sources of forecast errors inherent in the fixed coefficients models. A section called "applications" contains some tables comparing the performance of stochastic and fixed coefficients models. The authors conclude that forecasts can be improved by allowing all coefficients in economic relationships to vary over time. They also note that the predictive capability of a model is not necessarily improved by complexity.

Swamy, Conway, and LeBlanc cite Oakes in declaring that universal forecasting algorithms are doomed to failure because the future is not like the past. They anticipate our plans for an article in a forthcoming issue of the Journal to address the larger issue of nonlinear dynamics and prediction where systems of equations produce solutions so complex they appear to be random—in a word, chaos.

And what of the data we put into our models? Boxley examines the way we represent the structure of agriculture and the rural economy in our definitions and data management. He notes that the American Agricultural Economics Association has been struggling with definitions and concepts since at least 1972. Still, we overdescribe commercial agriculture and fail to provide adequate information on other aspects of rural life. Boxley concludes that information should be based on two distinct paradigms, one for production agriculture and another for rural resources.

Larson, in his brief article on intertemporal duality, reviews several types of problems for which this approach can be used, then states equations useful for planning problems with an important time dimension. In his comment on contemporaneous correlation, Babula shows that estimates of parameters pertaining to U.S./Canadian crops trade were sensitive to econometric procedure. Reining comments on another estimation problem, namely, measuring structural change in agriculture, and recommends an inexpensive, flexible regression procedure.

Several books receive sharp reviews in this issue. Schluter examines the collection on agricultural and food marketing edited by Kilmer and Armbruster and finds it less than it might have been. Hauver disagrees with Dovring's pessimism about multifactor productivity indexing by citing advances in duality theory and Tornqvist indexing.

Ribaudo, however, favorably reviews the Resources for the Future book on agricultural policy by supporting its argument that traditional commodity-based policy is inadequate to cope with today's agricultural and food problems. Tripaldi and Schor give qualified support to the Murdock-Leistritz book on the farm financial crisis.

Breimyer's appraisal of the anniversary tome from the American Agricultural Economics Association is mixed. Not surprising. A book with four editors and 57 authors is unlikely to be all good or all bad. Its chief deficiency, according to Breimyer, is that the subject of the book, the 21st century, was scarcely touched.

In this issue, we inaugurate a series of invited essays on professional issues in applied economics and related social sciences. It is fitting, I believe, that we begin with an essay on publishing in professional journals. Author of this thoughtful, upbeat essay is Peter Barry, editor of the American Journal of Agricultural Economics. There is much in his essay with which I readily concur, yet I do not share all his views. For example, while I agree that journal publication does figure substantially in the reward structure of the profession, I am concerned that it may do so by neglecting other vital professional responsibilities such as teaching and learning. A professional literature is a public good. We need to attend to the needs of readers as well as authors. The size and variety of a literature and its access should be professional assets, not liabilities. An expanding and specializing literature is a mark of professional achievement, but it does have its down side also. Barry's essay, whether you agree or disagree, contains much to think about. I urge you to read it, and comment on it.

Gene Wunderlich

Best Article Award

The ERS Administrator's Award for the best article in the Journal during fiscal year 1988 went to Fred Kuchler and Harry Vroomen for "Impacts of the PIK Program on the Farm Machinery Market," which appeared in the Summer 1987 issue.

Articles

Publishing in Professional Journals

Peter J. Barry

My assignments as a past editor of the *Western Journal of Agricultural Economics* and as current editor of the *American Journal of Agricultural Economics* have provided unique opportunities to view the publication and research activities in agricultural economics. As an editor, I could not help but become better acquainted with the diversity of the subject matter in our field, the issues addressed, and the people involved. One gains a profound appreciation for the quest for knowledge and the intellectual efforts of people, working individually or collectively, to add to this knowledge.

As in most endeavors, the people involved in the journal process, authors, reviewers, journal readers, editors, are strongly motivated by self-interest. Some observers have recently worried that journal publication reflects too much the self-interest of those involved, especially authors, and that professional interests have become secondary, that creativity and risk-taking in journal publication are stifled, and that gamesmanship by authors (some reviewers, too) has become too prominent. My impression is that such concerns are exaggerated but are nevertheless features of the journal environment.

Professional journals provide several key functions, according to a recent article by Lacy and Busch.[1] First, journals disseminate information about new ideas, methods, institutions, theories, data, or ways of approaching problem situations. They foster scientific inquiry, dialogue, and debate and become the primary means of advancing an area of knowledge.

Journals are "gatekeepers". They serve a quality control function by vouching for the scientific integrity of the work involved. The decision to publish based on formal reviews of manuscripts by experts and editorial staff is a vital part of this function.

Lacy and Busch write further that journals have been responsible for enforcing scientific norms in the creation of disciplinary knowledge. That means they exercise a fair and consistent application of "objective standards in a universalistic manner, organized skepticism, disinterestedness, communality, and emotional neutrality." Editors and reviewers do not legislate the normative criteria in their respective fields. Rather, they are entrusted to apply the accepted and commonly understood research values of their particular discipline.

They conclude that journal publication is a forum that confers professional recognition and other rewards, because the performance of scientists is largely judged by their publications. Publication plays a major role in a professional's career advancement. The criteria often are imperfect because administrators and other evaluators may place more emphasis on the number of journal publications and where they are published than on the value of the contribution to the field. This places a still greater burden on the journals to evaluate the value of the authors' contributions.

How have journal functions evolved in a changing intellectual environment? Scientists know that keeping up with new developments in their field is especially challenging. Specialty areas become more refined and fragmented and are subject to periodic changes. Methods of analysis become increasingly sophisticated. Mathematical techniques often appear to dominate and new ideas may sometimes appear to be based more on refinements and twists, or on tinkering with existing models and methods rather than on resolving current problems or understanding key economic relationships. The depth, scope, and complexity of agricultural economics have expanded considerably, and the competition among scientists to produce rather than consume new knowledge has grown as well.

At the same time, professional journals have taken on greater importance relative to bulletins, reports, and other types of publications in reporting new scientific knowledge. Journals, therefore, have assumed more responsibility in verifying the integrity of the work

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[1]W.B. Lacy and L. Busch, "Guardians of Science: Journals and Journal Editors in the Agricultural Sciences," *Rural Sociology*, Vol. 47, 1982, pp. 429-48.

and testifying to the productivity of individual scientists. In response, journal submissions have risen as has the number of journals. And, even within a discipline, journals vie for professional status.

These developments have created a more complex environment for each of the major participants in journal publication. Small wonder that authors have become more strategic in planning publication activities, considering the growth in analytical sophistication, more publication outlets, greater competition, high degrees of uncertainty about publication prospects, the typically lengthy process of journal publication, and the professional rewards at stake. Keeping one's publication pipeline full, diversifying publication outlets, planning the sequence of submissions, and engaging in more joint work are examples of the elements of a publication strategy.

Journal editors, staffs, and reviewers must scrutinize contributions closely, and journal readers must be more strongly equipped to understand, evaluate, and synthesize published work of varying degrees of technical sophistication. Many readers will not read a general journal from cover to cover. Rather, the tendency of scientists to specialize in subject matter areas and analytical techniques, and the availability of other journals, suggest that individual readers will be attracted to few articles in a single issue. Finally, more effort is needed by the scientific community to move the knowledge reported in journals into education, policy, and managerial channels for the benefit of various clientele groups.

My perception of journal publication is that the process works well. I do not sense that authors have been inhibited from taking risks, have had creativity stifled by concerns about career advancement, or have engaged in manuscript reviews with an eye on their own work. (It is the editor's job, of course, to manage the review process to avoid such conflicts of interest between authors and reviewers.)

I do not sense that authors engage in excessive gamesmanship by submitting the same article to several journals, by misleading editors about prior publication, by trying to guide the review process, or by arguing about editorial decisions. (I do, however, have a few interesting exceptions filed away.)

I do not feel that authors exploit the journal's review process to improve the quality of their own work. Rather, it is natural to expect that reviewers' comments and suggestions will contribute to the quality of research and the effectiveness of its presentation. In my own work, the assistance from anonymous reviewers has consistently proved helpful, and I view manuscript reviews as an integral part of the knowledge-creating process.

Nor do I believe that the journal publication process is prone to a high incidence of error in the accuracy and validity of published work. Nonetheless, the periodic publication of comments and replies and observations of authors' occasional self-discovery of errors during the publication process indicates that this is a matter of concern. This topic received considerable attention in articles in the *American Economic Review, Science,* and other media in which serious questions were raised about difficulties in replicating published work, high incidences of error, and the integrity of some authors.[2] Some journals have gone so far as to require authors to submit their data, computer programs, and statistical routines along with their manuscripts. These requirements vouch for the accuracy of the work and encourage greater self-scrutiny by the authors themselves. Clearly, these issues will continue to command attention.

All participants in agricultural economics publishing must continue to make the journal process function as effectively as possible. This effectiveness will grow from placing the proper functions of journals ahead of personal gain. By focusing on enhancing knowledge creation, through the collective efforts of individual scientists, we will continue to advance our field, serve our clientele, and, in the process, benefit ourselves.

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[2]For example, see W.G. Dewald, J.G. Thursby, and R. G. Anderson, "Replication in Empirical Economics: The Journal of Money, Credit and Banking Project," *American Economic Review,* Vol. 76, 1986, pp. 587-603.

The Stochastic Coefficients Approach to Econometric Modeling, Part III: Estimation, Stability Testing, and Prediction

P.A.V.B. Swamy, Roger K. Conway, and Michael R. LeBlanc

Abstract. *In this final article of our three-part series, we demonstrate why stochastic coefficients models are well suited to predict future variables. We analyze the forecasting problem and consider various criteria of prediction. If a forecaster must choose one from among several coherent predictors, then the choice should be the one with the best track record. Decomposing the forecast error shows that stochastic coefficients models can cover more possible sources of prediction error and correct for them. The empirical record shows that stochastic coefficients models can substantially reduce out-of-sample forecast errors more than fixed coefficients models. Our assessment of coefficient stability tests is: they are contradictory, misleading, and without empirical value.*

Keywords. *Stochastic coefficients, fixed coefficients, conditional expectation, Bayesian inference, coherence, estimation, prediction, stability tests.*

> *Editor's note: Part I: A Critique of Fixed Coefficients Models* appeared in Vol. 40, No. 2, Spring 1988. *Part II: Description and Motivation* appeared in Vol. 40, No. 3, Summer 1988.

Stochastic coefficients models are ideally suited to the problem of predicting future values of variables. We say ideally because such models cover almost all possible sources of prediction errors and introduce suitable corrections for each error. We also show that either parameter estimation or testing of hypotheses about parameters is a "halfway house" on the road to predicting future observations.

If the objective of estimation is forecast accuracy, then one should attempt to find an estimation procedure that yields predictions as close to actual realizations as possible. One should select the predictor that has the highest probability of taking values close to actual realizations.[1] It is impossible to derive predictors based on this general criterion. A necessary condition, however, for a predictor to take values close to actual realizations with the highest probability is that the mean square error (that is, the predictor's expected squared deviation from the actual realization) is a minimum.

Predictors with uniformly minimum mean square error typically do not exist, a difficulty that can be avoided by replacing the criterion of minimum mean square error with the criterion of minimum average mean square error.

The latter criterion selects a predictor if its expected squared deviation from a variable is a minimum. Minimum average mean square error predictors take the form of conditional expectations, which can be evaluated exactly if their true functional forms are known and if they do not depend on unknown parameters. Surfacing are problems in which the functional forms assumed for conditional expectations may not coincide with their true functional forms, and the errors of the estimates of the unknown parameters appearing in the assumed functional forms substantially affect the accuracy with which the desired values can be predicted.

Our approach, then, is to use stochastic coefficients models to specify conditional expectations. The motive for introducing stochastic coefficients models is the hope that such models can approximate true models better than fixed coefficients models. This hope is not without a methodological basis. If a functional form assumed for a conditional expectation is true, then it is appropriate to adopt the criterion of minimum variance unbiasedness for parameter estimation. This criterion can satisfy a necessary condition for maximizing the probability that a predictor can generate predictions that are close to the true conditional expectation. However, there still remains the problem of recognizing an operational unbiased predictor with minimum average mean square error

Swamy is a senior economist with the Board of Governors, Federal Reserve System, and adjunct professor of economics at The George Washington University (Washington, DC), and Conway and LeBlanc are agricultural economists with the Resources and Technology Division, ERS. The authors received valuable comments and help from James Barth, Charlie Hallahan, Arthur Havenner, Tom Lutton, Ron Mittelhammer, Peter von zur Muehlen, Nadine Loften, and Douglas McManus.

[1]Here, we use the term "predictors" to refer to a random variable's real-valued measurable functions that are used to predict the future values of the random variable. The term "predictions" refers to the values taken by these predictors. We use the term "estimators" to refer to the real-valued measurable functions of random variables which are used to guess the unknown true value of a fixed parameter.

(51).[2] Consequently, we may follow de Finetti's suggestion that the condition of coherence is the only minimum required condition one should impose on predictors.

The derivation of coherent predictions is no easy task. For example, Lane and Sudderth's method of deriving coherent predictions is difficult to use because we must specify a finitely additive probability distribution *(32)*. We may find a Bayesian procedure which gives coherent predictions, however, to approximate our opinions via a probability density function (pdf). Even this procedure may be complicated because the specification of a consistent set of prior probabilities of models and the prior pdf's for their parameters require a demanding exercise in self-interrogation. A Bayes procedure based on prior probabilities and prior pdf's can produce better forecasts than a non-Bayesian procedure in some cases. But, expressing our opinions about models and their parameters in the form of prior probabilities and pdf's, respectively, and checking their consistency, are very difficult tasks. The difficulty of checking the logical validity of models is, of course, common to both Bayesian and non-Bayesian methods.

We can apply exact or approximate Bayesian and non-Bayesian methods to generate predictions under some assumptions about the data-generating process. By dividing available data into fitting and forecast samples, we can use part of the data to generate predictions for the rest of the data, comparing these predictions with the realized values. The result of these comparisons can guide the choice of models in other situations that share common features with the environment, resulting in the data used in the comparisons. Our experience with such comparisons suggests that time-varying and stochastic modeling of regression slopes may contribute to improved forecasts. These forecasts become useful in analyzing sources of so-called coefficient instability, predicting uncertainty that may arise in conventional models.

The Forecasting Problem[3]

We are concerned with the following prediction problem: we want to predict the value y_{T+s} that would be taken by a variable y_t^* in some future period s after T, where T is the terminal period of the currently available sample observations on y_t^*. We will make this prediction having T+s observations up to time T+s on a vector x_t^* of K variables that are related to another variable y_t^* and also having T observations on y_t^* up to time T.[4]

[2]Italicized numbers in parentheses cite sources listed in the References at the end of this article.

[3]Several sections in this article are based on *(51)*.

[4]We distinguish a random variable from its value by an asterisk. For example, y_t is the value taken by the random variable y_t^* in period t.

Formalizing this point prediction problem involves a random variable y_t^* taking on values in a sample space, according to a distribution which is assumed to belong to a family. The currently available sample observations $y = (y_1, y_2, \ldots, y_T)'$ on $y^* = (y_1^*, y_2^*, \ldots, y_T^*)'$ constitute the data. We may also have observations on variables with the symbol $x_t^* = (x_{1t}^*, x_{2t}^*, \ldots, x_{Kt}^*)'$. The observations x_t, $t = 1,2, \ldots, T+s$, on these variables are also part of the data when the distribution of y_t^* is determined by its own past values and by current and past values of x_t^*.

The problem is the determination of a suitable predictor, that is, a real-valued function $\hat{y}_{T+s}$ defined over the sample space, of which it is hoped that $\hat{y}_{T+s}(y^{*\prime}, x^{*\prime})$ will tend to be close to the unknown value y_{T+s}. The value $\hat{y}_{T+s}(y', x')$ taken on by $\hat{y}_{T+s}(y^{*\prime}, x^{*\prime})$ for the observed value $(y', x')'$ of $(y^{*\prime}, x^{*\prime})'$ is then the forecast of y_{T+s}, which will be our educated guess for the unknown value. We say that a predictor is operational if it does not involve any unobservable quantities.

A best predictor $\hat{y}_{T+s}$ should be sufficiently close to the actual realization y_{T+s} and because $\hat{y}_{T+s}$ is a random variable, the value y_{T+s} is covered by an interval of values which $\hat{y}_{T+s}$ takes with a high probability. To make this requirement precise, we specify four measures of closeness of (or distance from) a predictor to y_{T+s}.

Criteria of Prediction

Swamy and Schinasi extend well-known criteria used in point estimation to the problem of point prediction as follows *(51)*:

(a) *Criterion of highest concentration:* A predictor $\hat{y}_{T+s}$ of the actual value y_{T+s} is better than any other predictor $\tilde{y}_{T+s}$ when:

$$\begin{aligned} &\mathrm{pr}(y_{T+s} - \lambda_1 < \hat{y}_{T+s} < y_{T+s} + \lambda_2) \geq \\ &\mathrm{pr}(y_{T+s} - \lambda_1 < \tilde{y}_{T+s} < y_{T+s} + \lambda_2), \end{aligned} \tag{1}$$

for all possible values of λ_1 and λ_2 in a chosen interval $(0, \lambda)$ and for all possible realizations y_{T+s}. Here, pr denotes "probability."

(b) *Minimum mean square error:* Swamy and Schinasi show that a necessary condition to satisfy criterion 1 for all λ and for all y_{T+s} is:

$$E(\hat{y}_{T+s} - y_{T+s})^2 \leq E(\tilde{y}_{T+s} - y_{T+s})^2, \tag{2}$$

with the inequality being strict on a set of a positive Lebesgue measure, that is, the mean square error of $\hat{y}_{T+s}$ about the actual realization y_{T+s} is a minimum.

(c) *Pitman's nearness (PN):* A predictor $\hat{y}_{T+s}$ is nearer to the value y_{T+s} than another predictor $\tilde{y}_{T+s}$ when:

$$\text{pr}[L(\hat{y}_{T+s}, y_{T+s}) < L(\tilde{y}_{T+s}, y_{T+s})] > 1/2, \tag{3}$$

where the loss function, $L(\hat{y}_{T+s}, y_{T+s})$, represents the consequences of predicting y_{T+s} by a value of $\hat{y}_{T+s}$.

Swamy and Schinasi state two sets of sufficient conditions for a predictor to be nearer to y_{T+s} than another predictor in the PN sense.

(d) *Minimum average mean square error:* A predictor $\hat{y}_{T+s}$ is called the minimum "average mean square error" predictor if it minimizes:

$$E(\hat{y}_{T+s} - y^*_{T+s})^2. \tag{4}$$

If we wish to predict y_{T+s} from a Borel measurable function $f(y^*)$, say, of y^*, then among all such functions of y^* with finite second moment, that which minimizes the average mean square error 4 with $\hat{y}_{T+s} = f(y^*)$ is the conditional expectation of y^*_{T+s}, given $y^* = y$, denoted by:[5]

$$f(y) = E(y^*_{T+s} \mid y). \tag{5}$$

When $\hat{y}_{T+s} = E(y^*_{T+s} \mid y)$, the average mean square error 4 reduces to the average conditional variance of y^*_{T+s}, given y.

Criteria (a), (b), and (c) are based on distances from predictors to actual realizations, while criterion (d) is based on distances from predictors to y^*_{T+s}, which is a random variable. In the definition of criteria (b) and (d), attention has been implicitly restricted to predictors with finite variance, because otherwise the problem of minimizing the (average) mean square error does not arise. Predictors with infinite variance violate the necessary condition 2 and so they cannot satisfy the criterion of highest concentration (criterion 1). In fact, predictors satisfying either the criterion of highest concentration or the criterion of minimum mean square error (criterion 2) do not generally exist. For this reason, the minimum average mean square error criterion 4 is the one used extensively in the econometric literature. However, minimum average mean square error predictors sometimes have infinite mean square error. For example, the forecast y_{T+s-1} is a minimum average mean square error forecast if y^*_t follows a random walk, $y^*_t = y^*_{t-1} + a^*_t$, $t = 0, \pm 1, \pm 2, \ldots$, where a^*_t is a white noise error term with zero mean. For this process, $E(y^*_{T+s} \mid y_{T+s-1}) = y_{T+s-1}$, and $E(y_{T+s-1} - y^*_{T+s})^2$ is finite when viewed as the average conditional variance of y^*_{T+s}, given y_{T+s-1}, and is infinite when viewed as an unconditional mean square error of y^*_{T+s} about y_{T+s-1}. The distinction between criteria (b) and (d) is not clearly explained in the econometric literature.

Criterion (c) is different from criteria (b) and (d) in that attention is also given to predictors with infinite variance. Keating and Mason give examples of predictors that are good in the PN sense *(27, 28)*.

The result expressed in equation 5 is theoretically important but has little practical use unless one knows the true functional form of the conditional expectation of y^*_{T+s}, given y. A conditional expectation that does not exist, however, could not have generated our data. Conditions for the existence of various forms of conditional expectations are different *(52)*. One usually cannot verify the truth of these conditions, and the best one can do is to argue (from coherent economic theories, for example) that, in many cases, one would expect $(y^*_{T+s}, y^{*\prime})'$ to follow a distribution which implies the existence of the conditional expectation (equation 5) of particular form *(49)*.[6] The first step in any statistical method of generating predictions is to formulate a statistical model about the data-generating process. The distribution implied by this model is the one assumed for $(y^*_{T+s}, y^{*\prime})'$.

If the vector $(y^*_{T+s}, y^{*\prime})'$ is jointly normal, then the conditional expectation in equation 5 can be expressed as:

$$E(y^*_{T+s} \mid y) = Ey^*_{T+s} + \text{cov}(y^*_{T+s}, y^{*\prime})\,[\text{var}(y^*)]^- \cdot (y - Ey^*), \tag{6}$$

where $\text{cov}(y^*_{T+s}, y^{*\prime}) = E(y^*_{T+s} - Ey^*_{T+s})(y^* - Ey^*)'$, and $[\text{var}(y^*)]^-$ is any generalized inverse of the covariance matrix of y^* *(36,* p. 522). These variances and covariances may be time dependent if the variable $(y^*_{T+s}, y^{*\prime})'$ is nonstationary.

Conditions other than normality may also be used to derive the predictor, equation 6, with the minimum average mean square error property. Specifically, Chipman *(6,* pp. 603-5) proved that the predictor (equation 6) has the minimum average mean square error within the class of linear (affine) predictors of y_{T+s} whenever $(y^*_{T+s}, y^{*\prime})'$ is not normal but possesses finite second-order moments. So, one may be tempted to conjecture that only normal distributions give linear predictors with the minimum average mean square error property in linear regression. While this conjecture is not true without further conditions, it is true for most practical purposes, as rigorously proved by Goel and DeGroot *(19,* p. 899) and Rao *(38)*.

[5]See *(36,* p. 264) for a proof of this statement.

[6]Unlike incoherent theories, coherent theories whose premises are not contradictory can be true. The intuitively appealing concept of evidence stating that under no hypothesis, H, shall there be a high probability of outcomes being interpreted as strong evidence against H is useless unless our hypotheses are grounded in coherent economic theories.

Where the mean vector, $E(y^*_{T+s}, y^{*\prime})'$, is unknown (which seems usual) and the covariance matrix of $(y^*_{T+s}, y^{*\prime})'$ is known (which seems unusual), Goldberger, Swamy and Mehta, and Harville have minimized criterion 4, subject to the restriction that the predictor $\hat{y}_{T+s}$ is equal to the homogeneous linear function, $c'y^*$, where c is a T×1 vector of constants, and to the unbiasedness restriction that $E\hat{y}_{T+s}$ is equal to the mean assumed for y^*_{T+s} *(20, 23, 47)*.[7] The predictor that comes out of this constrained minimization procedure is called the minimum variance linear (homogeneous) "unbiased" predictor, and it is the same as the predictor expressed in equation 6 with Ey^*_{T+s} and Ey^* replaced by their respective minimum variance linear (homogeneous) unbiased estimators. The minimum variance linear "unbiased" predictor of y_{T+s} will coincide with the minimum variance "unbiased" predictor of y_{T+s} in the normal case but not with the conditional expectation in equation 5, even in this case.

Swamy and Schinasi show that the criterion of minimum variance unbiasedness satisfies a necessary condition for maximizing the probability that a predictor generates predictions close to its expected value *(51)*. No guarantee provides that actual realizations will be close to this expected value. The expected value of the minimum variance linear "unbiased" predictor of y_{T+s} will not coincide with the true expected value of y^*_{T+s} if the unbiasedness restriction is erroneous.[8] Imposing erroneous unbiasedness restrictions may have the undesirable consequence of yielding highly inaccurate forecasts.

Conditions 1 and 2 logically lead to the criterion of minimum mean square error in satisfying a necessary condition for maximizing the probability with which a predictor takes values close to actual realizations. If condition 2 is true, then it follows that for at least one value of y^*_{T+s} the inequality 1 is true but not necessarily for all possible values of y^*_{T+s} (*36*, p. 96). This result shows that a predictor, $\hat{y}_{T+s}$, which minimizes the mean square error, $E(\hat{y}_{T+s} - y_{T+s})^2$, for all values of y^*_{T+s} is useful if it satisfies the inequality 1 for those values of y^*_{T+s} which actually occur. Unfortunately, such a predictor does not exist, as shown in the statistics literature (*34*, p. 5). Nevertheless, comparing 2 with 4 shows that the conditional expectation of y^*_{T+s}, given a realization of y^*, nearly satisfies condition 2 if the true conditional distribution of y^*_{T+s}, given $y^* = y$, is sufficiently tight around its mean value. Therefore, a necessary condition for obtaining accurate forecasts is that we specify and evaluate accurately the true conditional expectation of y^*_{T+s}, given $y^* = y$. Perhaps, we can better satisfy this necessary condition if we work with stochastic coefficients models rather than fixed coefficients models. Again, any rigorous derivation of an econometric model using probability calculus naturally leads to a stochastic coefficients model unless severe restrictions are imposed on derivation. True models are better approximated by stochastic coefficients models than by fixed coefficients models, particularly when the premises of the latter are contradictory.

If we are interested in satisfying condition 2, why do we need condition 3? Our interest in the criterion of PN is justified by the following observations:

- PN is an intrinsic measure of acceptability *(27)*.
- Sufficient conditions can be found for satisfying the criterion of PN, whereas only a necessary condition can be found for satisfying the criterion of highest concentration 1.
- Keating and Mason's results demonstrate that neither mean square error nor PN should reign exclusively in the comparison of estimators (*27*).

Fixed and Time-Varying Coefficients Approaches

The true functional form of equation 5 is unknown, so a functional form for equation 5 must be assumed. The usual practice among econometricians is to presume that for every t, y^*_t follows the reduced-form model:

$$y^*_t = x_t'\pi + \epsilon^*_t \quad (t = 0, \pm 1, \pm 2, \ldots), \tag{7}$$

with fixed coefficients so that the minimum average mean square error linear predictor of y_{T+s} is:

$$x'_{T+s}\pi + w'\frac{V^{-1}}{\sigma^2}(y - X\pi), \tag{8}$$

where $Ey^*_{T+s} = x'_{T+s}\pi$, $Ey^* = X\pi$, $\text{cov}(y^*_{T+s}, y^{*\prime}) = w'$ and $\text{var}(y^*) = \sigma^2 V$ are implied by the assumptions underlying model (7) with fixed x_t. This predictor has the minimum variance within the class of linear "unbiased" predictors of y_{T+s}, if π in both the terms of the predictor 8 is replaced by $\hat{\pi} = (X'V^{-1}X)^{-1}X'V^{-1}y^*$. Several forms of w and V are given in (*25*, chaps. 8 and 11). For suitable definitions of x'_{T+s} and X, the predictor

[7] This unbiasedness restriction ensures that both the distribution of $\hat{y}_{T+s}$ and the distribution assumed for y^*_{T+s} are located at the same value so that their variances are comparable. It differs from the unbiasedness definition: $E_\theta(\hat{\theta}) = \theta$ for all $\theta \epsilon \Theta$, where $\hat{\theta}$ is an estimator of the fixed parameter θ and Θ is the parameter space.

[8] The unbiasedness restriction, $E\hat{y}_{T+s} = Ey^*_{T+s}$, is erroneous if the assumed functional form for the mean of y^*_{T+s} is different from the true form.

8 also represents the minimum average mean square error linear predictor of an element of a vector variable following a vector autoregressive (VAR) model. If equation 7 represents a univariate autoregressive model, then X consists of lagged y's, and w can be equal to 0. The vector w can be zero if equation 7 represents a regression model with a serially uncorrelated error term.

The predictor 8 will not give accurate forecasts in the case where the slopes of the function 7 change over time. The following model, developed in *(48)* may be appropriate:

$$y_t^* = x_t'\Pi z_t + x_t'J\xi_t^* \quad (t = 0, \pm1, \pm2, \dots). \tag{9}$$

(For an explanation of these symbols, see (*54,* part II)).

When this model is appropriate, the minimum average mean square error linear predictor of y_{T+s} is:

$$x_{T+s}'(z_{T+s}'\otimes I)\,\mathrm{vec}(\Pi) + x_{T+s}'J\Phi^s\Sigma_{\xi T}'(I\otimes J')D_x'\Sigma_y^{-}\,(y - D_xZ_e\,\mathrm{vec}(\Pi)), \tag{10}$$

where the first term equals Ey_{T+s}^*, $Ey^* = D_xZ_e\mathrm{vec}(\Pi))$, $\mathrm{cov}\,(y_{T+s}^*, \; y^{*\prime}) = x_{T+s}'J\Phi^s\Sigma_{\xi T}'(I\otimes J')D_x'$, and $\mathrm{var}(y^*) = \Sigma_y$ are implied by the assumptions underlying model 9 with "fixed" x_t and z_t (*54,* p. 27). This predictor becomes the minimum variance linear "unbiased" predictor of y_{T+s} if vec(Π) in both the terms of the predictor 10 is replaced by $\mathrm{vec}(\hat{\Pi}) = (Z_e'D_x'\Sigma_y^{-1}D_xZ_e)^{-1}\,Z_e'D_x'\Sigma_y^{-1}y^*$ whenever Σ_y is nonsingular.

The model in 9 provides a useful approach for the decomposition of forecast error sources. Partition several of the vectors of 9 as follows:

$$x_t' = (1, x_{2t}'),\; z_t' = (1, z_{2t}'),\; J = (J_1, J_2)',\; \Pi = (\pi_1, \Pi_2). \tag{11}$$

Model 9 may then be expressed as the sum of terms similar to model 7 and additional terms involving x_{2t} and z_{2t}.

$$y_t^* = x_t'(\pi_1, \Pi_2)\begin{pmatrix}1\\ z_{2t}\end{pmatrix} + (1, x_{2t}')\begin{pmatrix}J_1'\xi_t^*\\ J_2'\xi_t^*\end{pmatrix}$$

$$= x_t'\pi_1 + x_t'\Pi_2z_{2t} + J_1'\xi_t^* + x_{2t}'J_2'\xi_t^*. \tag{12}$$

An estimated version of the fixed coefficients model 7 implies a forecast of y in some future period s after T, given by $\hat{y}_{T+s}$,

$$\hat{y}_{T+s} = \hat{x}_{T+s}'\hat{\pi} + \hat{\epsilon}_{T+s}, \tag{13}$$

where $\hat{x}_{T+s}'\hat{\pi}$ and $\hat{\epsilon}_{T+s}$ are some estimators or predictors of the first and second terms on the right-hand side of the predictor 8.

The forecast error (the difference between the predictor, $\hat{y}_{T+s}$, and the future realization, y_{T+s}) that arises from using a fixed coefficients model when model 12 is true, may be decomposed as:

$$\begin{aligned}\hat{y}_{T+s} - y_{T+s} = {} & \hat{x}_{T+s}'(\hat{\pi} - \pi_1) \\ & + (\hat{x}_{T+s}' - x_{T+s}')\pi_1 + (\hat{\epsilon}_{T+s} - J_1'\xi_{T+s}) \\ & - x_{T+s}'\Pi_2z_{2T+s} - x_{2T+s}'J_2'\xi_{T+s},\end{aligned} \tag{14}$$

which, in order of appearance, is the sum of (1) a linear combination of the sampling errors of the coefficient estimates, (2) a linear combination of the errors in predicting future values of the independent variables, (3) the error in predicting stochastic shifts in the intercept, (4) the failure to predict deterministic shifts in regression intercept and slopes, and (5) the failure to predict stochastic shifts in regression slopes. Except for (2), all these forecast error sources are accounted for when equation 9 is used. Observe that an accounting of forecast error sources based on an estimate of equation 7 is limited to (1) and (3). The remaining error sources cannot be diagnosed using fixed coefficients models. The error resulting from (2) is, of course, beyond the reach of any of the equations 7 and 9, because it originates from errors in forecasting exogenous events and/or comes from observation, sampling, and measurement deficiencies.

One persistent problem in applied economic forecasting has been the recurrence of forecast drifts causing selected model equations to drift away from later historical realizations. The conventional add-factoring of intercepts has not always proved satisfactory, especially in cases of suspected nonstationary regression slopes. We have shown that one role of the z_2 variables in equation 12 is to account for sources of coefficient nonstationarities. Equation 12 accounts for movements in coefficients that are caused by movements in certain observable variables suggested by theoretical considerations but neglected in equation 7. In diagnostic terms, if the z_2 variables are eliminated, then forecast error interpretations are limited because forecast errors cannot be based on errors in predicting deterministic shifts in regression slopes (see equation 14). Equation 12 is useful for distinguishing between errors arising from intercept instability, amenable to add-factor solutions, and errors arising from other sources.

The list of potential sources of errors in equation 14 is exhaustive. Although add-factoring (the judgmental adjustment of intercepts to realign errant equations to fit current data) has been useful, the exclusive focus on intercept instability, that is $(\hat{\epsilon}_{T+s} - J_1'\xi_{T+s})$, by add-factoring may mean that important sources of forecast error remain unaccounted for. An econometric methodology with built-in features for measuring all coefficients variation, as in equation 9, however, could feasibly lend itself to being used as a diagnostic

tool for ascertaining all sources of equation instability, $x'_{T+s}(\Pi_2 z_{2T+s} + J\xi_{T+s})$. An estimate of equation 9 would yield an allocation of the total uncertainty over all components of an equation, as shown in equation 12, permitting quick investigation of the likely sources of future equation volatility.

If alternative policy regimes have parametric implications for the behavior of the economy, as suggested by the so-called Lucas critique, a time-varying stochastic coefficients approach may provide a means for anticipating consequences $x'_{T+s}\Pi_2 z_{2T+s}$ of alternative conjectured policy assumptions, not available with conventional fixed coefficients techniques, whenever the z_{2t} elements include observable policy variables.

Without a doubt, equation 7 is simpler to work with than model 9 because the second and fourth terms appearing on the right-hand side of equation 12 are ignored in equation 7. Even though including these terms complicates our models and possibly makes our parameter estimates imprecise and nonunique, we have no choice except to include them if model 7 does not give useful forecasts. No logical principle warrants excluding these terms because no one knows for sure that these terms are absent from the true model. We later show why the prediction principle advocated by Zellner (*61,* p. 32) and others cannot conclusively reject equation 9 in favor of equation 7.

Estimation Procedures

The predictors 8 and 10 are not operational because they involve unknown parameters. To obtain computable forecasts, we need the estimates of these parameters. The vector π or vec(Π), if fixed, can be estimated by one or more of the following procedures:

- The least squares procedure;
- The generalized least squares procedure based on an estimated error covariance matrix;[9]
- A fully or partially restricted reduced-form procedure that fully or partially accounts for the connection between π and the coefficients of a structural model;
- A Bayes procedure;
- Shrinkage estimators; and
- Robust procedures.

[9]Swamy and Tinsley's estimate of the error covariance matrix for model 9 may be singular (*48*), in which case Paige's numerically stable and efficient algorithm based on matrix decompositions should be used for estimating model 9 (*31*).

The corresponding methods of estimating w, V, σ^2, and the variances and covariances in equation 10 are also available. Several methods of estimating w, V, and σ^2 are summarized in (*25*) and a method of estimating variances and covariances in equation 10 appears in (*48*).[10]

Swamy and Schinasi show that, if all the unknown parameters in equations 8 and 10 are replaced by their respective sample estimates, then we cannot in practice recognize an operational "unbiased" predictor with minimum variance in small samples (*51*). They also show that a universally preferred choice among different estimation procedures for equations 7 and 9 is not possible based on either the exact finite sample distribution theory or the asymptotic theory.

Akaike's Information Criterion

Akaike has derived from information-theoretical considerations a probability density function (pdf) which may be expected to approximate the true pdf for a variable (*1, 2*). The criterion he has used to find this approximation is:

$$B(p, g) = -\int \frac{p(y)}{g(y)} \log\left[\frac{p(y)}{g(y)}\right] g(y)dy, \qquad (15)$$

where p(y) is the true pdf for a variable y^*, g(y) is an approximation to p(y), and the integration is over the entire range of y^*. Clearly, this criterion can be written as:

$$B(p, g) = E\log g(y) - E\log p(y) \leq 0, \qquad (16)$$

where the expectation is with respect to the true distribution of y^*.

Because the quantity on the right-hand side of equation 16 is nonpositive, when $\int_{-\infty}^{\infty}[p(y) - g(y)]dy \geq 0$, as shown by Rao (*36,* p. 59), the greater the value of Elogg(y) is, the closer the pdf g(y) is to the true pdf p(y) in the sense of B(p,g). However, the statement that the unknown true pdf, p(y), can be well approximated by g(y) if and only if g(y) maximizes Elogg(y) is useless as it stands. Deciding whether the condition is or is not satisfied or taking the expectation of logg(y) with respect to p(y) is impossible without knowing the family of pdf's which covers the true pdf for y^* as a special case. The maximum likelihood method is applied to a family of pdf's for this reason, which

[10]Swamy and Tinsley's (*48*) method of estimating variances and covariances extends Swamy's earlier work (*45, 46*), which does considerably more than Chow's (*7,* p. 340; *8,* p. 1,237) perfunctory description of it as a "survey." Reinsel (1982, 1984) also presents estimators and predictors (*39, 40*). He, however, basically repeats the results recorded earlier in the above papers (*47*).

presumably covers the true pdf as a special case. The strict inequality in equation 16 is an important step in proving the consistency of maximum likelihood estimators (*29,* p. 891). The only explicit statements about the interpretation of a pdf like p(y) in criterion 15 that we have found are in the applications of criterion 15, where p(y) is thought of as the pdf of the unknown true distribution. Does this mean that distributions which do not possess pdf's cannot be true? A singular normal distribution does not possess a pdf except on a subspace. Prior distributions satisfying Shiller's smoothness restrictions do not possess pdf's on the entire parameter space (*26, 57*).[11] Even though stable distributions have pdf's, these pdf's are generally expressed only as infinite series, which are not easy to work with. Any of these distributions can be true. We should not say, then, that p(y) in criterion 15 is the pdf of the unknown true distribution. If p(y) is restricted *a priori* to belong to a particular family of pdf's, then criterion 15 may have the same defects as the maximum likelihood criterion (*53,* p. 8). For example, if we assume that p(y) belongs to the family of pdf's implied by a mixed autoregressive, moving average model of finite but unknown order, then criterion 15 does not lead to consistent estimates of the order unless it is modified, as in (*22*). (See *(43)*).

Swamy and von zur Muehlen have developed some sufficient conditions for the existence of different families of distributions *(52)*. Logic permits us to say only that these families are true if their sufficient conditions are true. But, no one can determine the truth of these sufficient conditions, assuming they are coherent. Our beliefs about these sufficient conditions may be expressed as subjective probabilities, which may then be transformed consistently into subjective probabilities on individual distributions (*52*). The defect of criterion 15 is that it is unable to take into account such probabilities.

A justification of criterion 15 rests on the belief that the entropy of a distribution is a good measure of uncertainty. Copas shows this belief is not correct in nonnormal cases by way of an example where a company is operating under much greater uncertainty in one of the two cases, though the entropies of distributions in the two cases are exactly the same (*15*). Copas wrote that this result arises as a direct consequence of the fact that the entropy of a distribution depends only on the distribution of the different heights of its pdf, paying no attention to the values of the variable at which these various heights are attained. Entropy can be, therefore, a very imperfect measure of statistical uncertainty. B(p, g) should not be used as a measure of the distance between g(y) and p(y) for this reason, regardless of any knowledge of the context and restrictions it puts on the shape of distribution one finds attractive.

A possible alternative reaction is to note that, when some conditions are satisfied, equation 15 provides useful forecasts. A set of such conditions is provided by Shibata (*44*). He has proved that if p(y) is determined by an autoregressive process of infinite order and if g(y) is determined by an autoregressive process of finite order K(<T)(AR(K)), where the order K is selected so as to maximize Elogg(y), or some other modification of Elogg(y), then an asymptotic lower bound is attained in the limit for the average mean square error of an estimated conditional mean of AR(K). This result is a large sample analog of the exact finite sample result that the conditional mean of AR(K) is a minimum average mean square error predictor if AR(K) is the true model. The key assumption used by Shibata is that the order of the autoregression determining the true pdf is infinite. Statisticians who believe in the principle of parsimony or simplicity assign to such an assumption the zero probability of being true. (See *(43)*).

It is difficult to determine whether or not Shibata's demonstration constitutes an argument against the information criterion 15, or against the principle of parsimony or simplicity, or against de Finetti's (*16*) condition of finite additivity. In any case, autoregressive models of finite or infinite order clearly ignore sources (4) and (5) of forecast errors described in equation 14. One cannot be sure that these sources are absent in any forecasting situation. Garcia-Ferrer, Highfield, Palm, and Zellner's results showed that autoregressive models of order 3 for annual real output growth rates of nine countries did not generally result in lower root mean square forecast errors relative to naive models, so relying solely on Shibata's theoretical result is difficult (*17*).

Importance of Comparing Different Predictors

Oakes has proved that no universal algorithm guarantees accurate forecasts forever, so any attempt to prescribe a single forecasting procedure, applicable to all empirical situations, must be unsatisfactory (*35*). No agreement of the values taken by a predictor (based solely on the data known up to the current period) with the actuals for a finite past time period could possibly imply that the values of the predictor would agree with the actuals in the future. Past success does not guarantee future success. If we knew only that a predictor had produced accurate forecasts in a past period, we could not guarantee that any future

[11]There are applications of equation 15, where there is no mention of these points.

forecasts generated by the predictor would be sufficiently accurate, because some predictors exist for which the initial values do not control the future values.

For this reason, de Finetti set up minimal criteria that forecasts should be coherent based on data currently available (*16*). One predictor is as valid as any other, if they all satisfy the requirements for coherence based on what knowledge is available. A predictor that conforms to probability calculus or does not violate any of the probability laws is coherent. This means only that de Finetti's concept of coherence prohibits the use of any contradictory restrictions or premise that is inconsistent relative to the axioms of probability theory. For example, if the premises of equation 7 are contradictory, then we cannot obtain coherent forecasts by using that model.

If a forecaster must choose one predictor from among several coherent ones, a likely choice would be the one with the best track record. The forecast can represent, at most, a measure of the confidence with which one expects that predictor to forecast an event based on currently available evidence, and not based on information yet to be observed. Obtaining useful codification of statistics that yields a satisfactory predictor selector for all people in all settings is impossible. Each experimenter must choose among various coherent predictors by comparing their past forecasting performance.

A Coherent Approach to Prediction

We consider a Bayesian solution to the problem of finding the entire predictive distribution. Jeffreys' book (*24*) is mainly responsible for the following Bayesian approach in Zellner's (*60*, pp. 306-17) and Geisel's (*18*) seminal work on comparing models. Given our beliefs in the form of a finite set of exhaustive and mutually exclusive models, $M_1, M_2, \ldots, M_n$, about the process that has generated the values of the variable y^*, we can compute the marginal probability density function (pdf) for y^* implied by the ith model by:

$$p(y \mid M_i) = \int_{R_{\theta_i}} p(y \mid \theta_i, M_i) p(\theta_i \mid M_i) d\theta_i, \tag{17}$$

where θ_i is the vector of parameters appearing in M_i; $p(y \mid \theta_i, M_i)$ is the conditional pdf for y^* given θ_i and M_i; $p(\theta_i \mid M_i)$ is the prior pdf for θ_i; and R_{θ_i} is the range of θ_i. Let $pr(M_i)$ denote the prior probability of M_i being true. When a particular value of the random variable y^*, say y, is observed, we may employ Bayes' theorem to revise the prior probability $pr(M_i)$ to become the posterior probability, that is:

$$pr(M_i \mid y) = \frac{pr(M_i)p(y \mid M_i)}{\sum_{i=1}^{n} pr(M_i)p(y \mid M_i)},$$

$$= \frac{pr(M_i)p(y \mid M_i)}{p(y)} \quad (i = 1, 2, \ldots, n). \tag{18}$$

We may derive the predictive pdf by:

$$p(y_{T+s} \mid y) = \sum_{i=1}^{n} pr(M_i \mid y) p(y_{T+s} \mid M_i, y), \tag{19}$$

where

$$p(y_{T+s} \mid M_i, y) = \int_{R_{\theta_i}} p(y_{T+s} \mid \theta_i, M_i) p(\theta_i \mid M_i, y) d\theta_i.$$

The denominator of the ratio on the right-hand side of equation 18 is not equal to the unconditional pdf for y^* unless $M_1, M_2, \ldots, M_n$ are mutually exclusive and exhaustive. We did not violate any probability laws in deriving equation 19. In this sense, the predictive pdf, equation 19, is coherent. More important, equation 19 gives a coherent method of pooling the predictive pdf's given by different competing models of the same data-generating process, as long as the premises of any of these models are not contradictory.

If we use only one model, say M_1, and do not use all other models to generate the predictive pdf for y^*_{T+s}, then we set $pr(M_1) = 1$ and $pr(M_i) = 0$ for $i \neq 1$. For these values of $pr(M_i)$, it is obvious from 19 that $p(y_{T+s} \mid y) = p(y_{T+s} \mid M_1, y)$. Formulas 8 and 10 are based on the assumptions that pr(model 7) = 1 and pr(model 9) = 1, respectively. These assumptions are false if we view models 7 and 9 as approximations to the true model because any approximately true model is neither absolutely true nor absolutely false. Because we do not know of any models that are literally true, down to the last decimal point, some analysts feel that all models are false. Boland says this opinion is a self-contradiction (*5*, p. 179). We do not believe that self-contradiction is consistent with Bayesian *coherent* behavior. If we truly believe that all the models $M_1, M_2, \ldots, M_n$ considered in equation 18 are indeed false, then as coherent Bayesians, we should be saying that $pr(M_i) = 0$ for $i = 1, 2, \ldots, n$. Otherwise, we would be contradicting ourselves. If $pr(M_i) = 0$ for $i = 1, 2, \ldots, n$, then formula 18 is indeterminate. Our models can be true if we satisfy the necessary condition of logical validity, although we cannot establish their truth status.

Swamy and von zur Muehlen discussed probabilistic logic as a valid tool for scientific analysis and interpretation of causal relationships (*52*). This logic can be used to merely bound (rather than specify) $pr(M_i)$, if we have some beliefs about the sufficient conditions under which M_i is true. Thus, scientific beliefs are useful in quantifying $pr(M_i)$. The prior pdf's for θ_i must also be consistent with these beliefs. In this sense, $pr(M_i)$ is related to $p(\theta_i \mid M_i)$. Because we do not know of any model that is literally true, we should assign positive probabilities to more than one logically valid model. This assignment is warranted by the frequent disagreement among economists as to which model is superior to address a given issue. If any consensus that ignores all but one of the opinions expressed is not satisfactory, then it is reasonable to have more than one model with a positive probability of being true. The problem with the predictive pdf 19 is that coming up with an exhaustive and mutually exclusive set of models is difficult. The prior pdf's $p(\theta_i \mid M_i)$, $i = 1,2,\ldots,n$, which were selected based on considerations of mathematical convenience, may not be consistent with the values assigned to $pr(M_i)$, $i = 1,2,\ldots,n$, and may not represent anybody's beliefs. If we prefer model 7 to model 9 because the Bayesian analysis of model 7 is simpler than the Bayesian analysis of model 9, then our inferences are incoherent if the premises of model 7 are contradictory.

Linkage

Suppose that we have two different econometric models giving two different predictions of an unknown value y_{T+s}. We do not know which one to choose because we do not know which one of these two predictions will be closer to the actual value y_{T+s}. This is not unusual in economics. We will have more than two models giving us more than two predictions about the same value. If these models are not mutually exclusive and exhaustive, then we cannot use the previously discussed Bayesian approach. However, we can use the following non-Bayesian approach under certain conditions.

Let $\hat{y}_{1,T+s}, \ldots, \hat{y}_{m,T+s}$ be the "unbiased" predictors of y_{T+s} given by m different econometric models, and assume that we have reason to believe that the expected squared deviation of $\hat{y}_{1,T+s}$ from y^*_{T+s} is smaller than that of any $\hat{y}_{j,T+s}$ for $j = 2,\ldots,m$. Let $\tilde{y}_{T+s} = (\hat{y}_{2,T+s}, \hat{y}_{3,T+s}, \ldots, \hat{y}_{m,T+s})'$ and let $\iota = (1,1,\ldots,1)'$ be an $(m-1)\times 1$ vector of unit elements. Suppose that $\hat{y}_{1,T+s}$ is correlated with the predictor $(\tilde{y}_{T+s} - \iota\hat{y}_{1,T+s})$ with zero expectation. Then there exists an (m+1)th predictor whose expected squared deviation from y^*_{T+s} is smaller than that of $\hat{y}_{1,T+s}$.

This (m+1)th predictor is:

$$\hat{y}_{m+1,T+s} = \hat{y}_{1,T+s} - \mathrm{cov}(\hat{y}_{1,T+s}, (\tilde{y}_{T+s} - \iota\hat{y}_{1,T+s})') \cdot [\mathrm{var}(\tilde{y}_{T+s} - \iota\hat{y}_{1,T+s})]^{-}(\tilde{y}_{T+s} - \iota\hat{y}_{1,T+s}), \quad (20)$$

where all the variances and covariances are about y^*_{T+s} and for any matrix A, A^- denotes a generalized inverse of A. The predictor 20 results from making covariance adjustment in $\hat{y}_{1,T+s}$ with respect to the concomitant variable $(\tilde{y}_{T+s} - \iota\hat{y}_{1,T+s})$ with zero expectation (*37*, p. 359).

However, it is doubtful that the predictors $\hat{y}_{1,T+s}$ and $\tilde{y}_{T+s}$ given by different models suffering from different types of specification errors will be "unbiased". If $E\hat{y}_{1,T+s} \neq E\hat{y}_{j,T+s}$ for $j = 2,\ldots,m$, which seems likely, then the expected squared deviation of (20) from y^*_{T+s} will not be smaller than that of $\hat{y}_{1,T+s}$ because the predictor $(\tilde{y}_{T+s} - \iota\hat{y}_{1,T+s})$ with nonzero expectation is not a concomitant variable suitable for making covariance adjustment in $\hat{y}_{1,T+s}$, even when $\hat{y}_{1,T+s}$ is "unbiased". If we use sample estimates of the variances and covariances in place of their known values used in equation 20, then we are no longer in a position to claim that the expected squared deviation of 20 from y^*_{T+s} is always smaller than that of any $\hat{y}_{j,T+s}$ (*37*, p. 360). The predictor 20, based on estimated variances and covariances will be incoherent if the estimates violate any of the assumptions under which the constituent predictors, $\hat{y}_{j,T+s}$'s, are derived.

The difficulties presented by equation 20 are not encountered if we use equation 9 alone. Because equation 9 is a general model covering various fixed coefficients models as special cases (*54*) we can justify using this general model for predicting and abandoning the method of pooling the predictions of different fixed coefficients models, particularly when the premises of the fixed coefficients models are contradictory.

Stability Tests

Some econometricians would like to see some evidence against the stability of the coefficients of equation 7 before they admit that a version of equation 9 deserves their consideration. Stability tests are supposed to give such evidence. A brief description of these tests appears in (*30*, pp. 575-8). Based on our discussion of Birnbaum's confidence concept in Part I (*4, 53*), a full disclosure of statistical evidence takes the form $d_1^* =$ (reject H_0 in favor of H_1, α_I, β_{II}) or $d_2^* =$ (reject H_1 in favor of H_0, α_I, β_{II}), where H_0 = a null hypothesis, H_1 = an alternative hypothesis, α_I = the probability of type I error, and β_{II} = the probability of type II error. Can we come up with such disclosures about coefficients' stability?

We divide the available time series of length T on variables in equation 7 into G mutually exclusive subperiods, with m_1 observations in the first subperiod, and m_2 observations in the second subperiod, so that $\min(m_1, m_2, \ldots, m_G) > K$. Note that $\Sigma_{i=1}^{G} m_i = T$. Assuming that the coefficient vector π varies between subperiods but not within each subperiod, we can depict the observations as:

$$\begin{bmatrix} y_1^* \\ y_2^* \\ \cdot \\ \cdot \\ \cdot \\ y_G^* \end{bmatrix} = \begin{bmatrix} X_1 & 0 & \ldots & 0 \\ 0 & X_2 & \ldots & 0 \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ 0 & 0 & \ldots & X_G \end{bmatrix} \begin{bmatrix} \pi_1 \\ \pi_2 \\ \cdot \\ \cdot \\ \cdot \\ \pi_G \end{bmatrix} + \begin{bmatrix} \epsilon_1^* \\ \epsilon_2^* \\ \cdot \\ \cdot \\ \cdot \\ \epsilon_G^* \end{bmatrix}, \tag{21}$$

or more compactly as:

$$y^* = X\pi + \epsilon^*, \tag{22}$$

where for $i = 1, 2, \ldots, G$, y_i^* is a $m_i \times 1$ vector of observations on the dependent variable, X_i is a $m_i \times K$ matrix of rank K of observations on K independent nonstochastic variables, π_i is a $K \times 1$ vector of regression coefficients, ϵ_i^* is a $m_i \times 1$ vector of stochastic disturbances, $y^* \equiv [y_1^{*\prime}, y_2^{*\prime}, \ldots, y_G^{*\prime}]'$, $\pi \equiv [\pi_1', \pi_2', \ldots, \pi_G']'$, $\epsilon^* \equiv [\epsilon_1^{*\prime}, \epsilon_2^{*\prime}, \ldots, \epsilon_G^{*\prime}]'$, and X represents the block-diagonal matrix on the right-hand side of equation 21. The vector ϵ^* is assumed to have a normal distribution with mean zero and the covariance matrix Σ.

The null hypothesis of coefficient stability can be stated as:

$$H_0: \pi_1 = \pi_2 \ldots = \pi_G, \tag{23}$$

which can be expressed as:

$$R\pi = \begin{bmatrix} I & -I & 0 & \ldots & 0 & 0 & 0 \\ 0 & I & -I & \ldots & 0 & 0 & 0 \\ \cdot & \cdot & \cdot & & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & & \cdot & \cdot & \cdot \\ 0 & 0 & 0 & \ldots & I & -I & 0 \\ 0 & 0 & 0 & \ldots & 0 & I & -I \end{bmatrix} \begin{bmatrix} \pi_1 \\ \pi_2 \\ \cdot \\ \cdot \\ \cdot \\ \pi_{G-1} \\ \pi_G \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \\ \cdot \\ \cdot \\ \cdot \\ 0 \\ 0 \end{bmatrix}. \tag{24}$$

The statistics literature states that under the null hypothesis (23), the statistic, $(T-GK)/(G-1)K$ times

$$\frac{y^{*\prime}\Sigma^{-1}X(X'\Sigma^{-1}X)^{-1}R'[R(X'\Sigma^{-1}X)^{-1}R']^{-1}R(X'\Sigma^{-1}X)^{-1}X'\Sigma^{-1}y^*}{y^{*\prime}\Sigma^{-1}y^* - y^{*\prime}\Sigma^{-1}X(X'\Sigma^{-1}X)^{-1}X'\Sigma^{-1}y^*}, \tag{25}$$

is distributed as F with $(G-1)K$ and $T-GK$ degrees of freedom (*59*).

Suppose that we use statistic 25 to test the hypothesis 23 against the alternative hypothesis,

$$H_1: R\pi \neq 0, \tag{26}$$

and come up with the decision,

$$(\text{reject } H_0 \text{ in favor of } H_1, \alpha_I, \beta_{II}). \tag{27}$$

If the values of α_I and β_{II} in the statement 27 are sufficiently small, then the statement 27 provides strong but inconclusive evidence against coefficients' stability. The values of α_I and β_{II} in the statement 27, however, depend on the values of R and Σ used. If we use an incorrect value of either R or Σ, then the values of α_I and β_{II} will be incorrect and the evidence of the statement 27 will be misleading. Even if our assumptions about the forms of R and Σ are correct, and if we use a sample estimate of Σ in place of its known value used in the statistic 25, then we may not know the exact distribution of the statistic 25. If we use an asymptotic distribution of the statistic 25 to evaluate α_I and β_{II}, then due to the approximate nature of the values of α_I and β_{II}, the evidence in the statement 27 may be misleading. It is also possible that for some sample estimates (or *a priori* values) of Σ, $\alpha_I \geq 0.5$ and $\beta_{II} \geq 0.5$. In that case, the evidence in the statement 27 is worthless.

Other difficulties arise because 26 is not the only alternative hypothesis of interest. There is no guarantee that the coefficients of model 7 do not change in periods other than those specified by the hypothesis 26. Model 9 is appropriate if the alternative hypothesis that π changes at any or every t is true. Under this realistic alternative, the statistic 25 is not defined. Little reason exists to use the test statistic 25 if we want to test the null hypothesis 23 against this realistic alternative hypothesis. To divide a time series into event-conditioned subperiods as in equation 21, econometricians must have considerable knowledge of their data. One cannot be content with casual inspection of a few stereotyped measures, such as F values, as is common practice in much applied econometric work.

An alternative to the statistic 25 is the CUSUM or CUSUM-square statistic of Brown, Durbin, and Evans (*30,* pp. 576-8). These alternative statistics are based on recursive residuals which are not unique. We can get $T-K$ nonzero and K zero recursive residuals for model 7 (where T is the number of observations and K is the number of independent variables). We get different $T-K$ recursive residuals depending on which K of the T residuals are set equal to zero. Computing these residuals usually means Σ is arbitrarily set equal to $\sigma^2 I$. Therefore, the values of

α_I and β_{II} for the test of the null hypothesis in 23 against the alternative hypothesis that the coefficient vector of equation 7 changes at some unknown periods based on the CUSUM (or CUSUM-square) statistic depend on the value of Σ employed and also on which K of the T residuals are set equal to zero. For this reason, two different econometricians working with two different recursive residuals for the same model and data can come up with two different pairs of values of (α_I, β_{II}) for the CUSUM (or CUSUM-square) test. These pairs of values may give contradictory conclusions. It is also clear that the CUSUM (or CUSUM-square) test cannot detect shifts in coefficients in any period if we set the recursive residuals of that period equal to zero. This discussion and the discussion in the previous paragraphs show that the stability tests are not informative and can be misleading. By contrast, we can conclude that the coefficients of equation 7 are unstable if using equation 9 produces a noticeable and important improvement in forecasting performance relative to that of equation 7.[12]

Even in large samples, the CUSUM (or CUSUM-square) test does not give correct conclusions because, under the alternative hypothesis that π in 7 changes at some unknown periods, in some unknown manner, the power, $(1 - \beta_{II})$, of this test does not tend to 1 as the sample size tends to ∞. The basic difficulty is that the time-varying coefficients of equation 9, $\Pi z_t + J\xi_t$, are not consistently estimable. (See the uncertainty principle formulated by Swamy and Tinsley (*48,* p. 117).)The seductive danger of stability tests is that they pretend to a kind of relevance which their logical machinery cannot justify.

Some Applications

The authors have employed stochastic coefficients models before to forecast several economic variables for several time periods. The stochastic coefficients model they employed can be represented by:

$$y_t^* = x_t'\pi_1 + x_t'\epsilon_t^*, \tag{28}$$

where the $K \times 1$ vector ϵ_t^* is assumed to follow a first-order autoregressive vector process. Equation 28 is a restricted version of equation 9 obtained by zeroing the vector z_{2t} and setting $J\xi_t^*$ equal to ϵ_t^*.

[12]It is fair to point out that, like recursive residuals, forecasts based on asymptotically efficient estimators of the parameters of equations 7 and 9 may also be arbitrary when the asymptotic efficiency is defined as in Lehmann (*34,* p. 415). For example, when the solution of the likelihood equations for the parameters of equation 7 or 9 is not unique, asymptotically efficient likelihood estimators such as the one-step estimator suggested by Lehmann (*34,* p. 435) depend on a somewhat arbitrary initial estimator and need no longer agree with the maximum likelihood estimator even for large samples. However, the arbitrariness of asymptotically efficient estimators is of a very different nature from those of recursive residuals.

The reasons for considering equation 28 include:

- Employing model 28, though restrictive, is more general than model 7.
- Estimating model 28 does not need as much (magnetic) core on the computer as estimating model 9 needs.
- Comparing the forecasts of models 7, 28, and 9 shows whether proceeding in order of increasing complexity increases the accuracy of forecasts.
- Working with model 28 has computational benefits which produces fewer unknown parameters than model 9.

A natural approach to investigating the advantages and disadvantages of equation 28 is to apply Zellner's prediction principle (*61,* p. 32). Split the available time series into two nonoverlapping parts. The period of the first part is called the estimation (or fitting) period and the period of the second, the forecast period. Let $t = 1,2,\ldots, T$ be the fitting period and let $t = T+1, T+2,\ldots,T+n$ be the forecast period. Estimate model 28 and its fixed coefficients counterpart by using the first part and then use these estimated models along with the values of the independent variables for the forecast period to predict the values of the dependent variable for the forecast period without revising the parameter estimates. We call such forecasts nonsequential or *multi-step-ahead.*[13]

The authors have used Swamy and Tinsley's (*48*) method described in Part II of this article (*54*) to estimate equation 28. The maximum likelihood estimates of the parameters of equation 28 may not exist, and Swamy and Tinsley's iterative method of estimating these parameters is not guaranteed to converge, so we do not iterate on Swamy and Tinsley's method until convergence (*48*). Because equation 28 fits the sample values perfectly whenever an estimate of $x_t'\pi$ is added to the corresponding prediction of $x_t'\epsilon_t^*$,

[13]It is obvious that this procedure is not operational if the values of the independent variables for the period T+s are not available at the time of forecasting y_{T+s}. Since our purpose is to obtain separate estimates of the terms on the right-hand side of equation 14, we have to use these values of independent variables. Without separating the second of these terms from the rest, it is not possible to evaluate forecast errors arising from coefficients' instability. Forecasters are also interested in knowing the magnitudes of each term on the right-hand side of equation 14. Thus, we are solving here a problem which is more important than a practical forecasting problem.

If we estimate sequentially the fixed parameters using all past data prior to each of the forecast periods, $T+1, T+2, \ldots, T+n$, then we call the corresponding forecasts sequential or one-step-ahead. The primary purpose of Swamy and Schinasi's article (*51*) is to demonstrate that the one-step-ahead forecasts will not necessarily be closer to the realized values of the forecasted variable than the multi-step-ahead forecasts.There is no non-Bayesian theory which mandates prediction with sequential estimation.

measures of within-sample fits are useless to discriminate among the estimates obtained at different iterations of Swamy and Tinsley's procedure. To avoid overfitting, we choose estimates which minimize the root mean square forecast error:

$$[\frac{1}{n}\sum_{s=1}^{n}(\hat{y}_{T+s} - y_{T+s})^2]^{1/2},$$

where $\hat{y}_{T+s}$ is a forecast of y_{T+s} in some period s after the terminal period T of the fitting period. The root mean square forecast error is a generally good substitute for an averaged within-sample residual sum of squares. From L iterations of the Swamy and Tinsley procedure, we obtain L different estimates of the unknown parameters in model 28. Inserting these estimates into formula 10 furnishes L different predictions for each forecast period. We show these predictions by $\hat{y}_{T+s,i}$, $s = 1,2,\ldots,n$; $i = 1,2,\ldots,L$. These predictions give L different values for the root mean square forecast error: $[\frac{1}{n}\sum_{s=1}^{n}(\hat{y}_{T+s,i} - y_{T+s})^2]^{1/2}$, $i = 1,2,\ldots,L$. We select the estimates of the unknown parameters by minimizing these L values.[14] We use these minimum root mean square forecast error estimates to forecast the values of y_t^* beyond the period T+n. The sample beyond the period T+n is used to compare the forecasting performance of an estimated stochastic coefficients model with those of other models.

To estimate the fixed coefficients counterpart of equation 28, Swamy and his co-authors considered both the classical least squares procedure and an approximate generalized least squares procedure based on a sample estimate of the error covariance matrix. They also applied approximate Bayes and ridge-type shrinkage estimators to equation 7 with and without serial correlation in the error term. These estimates allow us to evaluate the corresponding minimum average mean square error predictors for the forecast period, T+1, T+2, . . . ,T+n, and the root mean square forecast errors of these predictions are computed. We obtain one root mean square forecast error for each fixed coefficients estimator. We choose the estimates of the fixed coefficients corresponding to the smallest of these root mean square forecast errors.

Table 1 shows the results of the authors' computations. Use of time-varying, stochastic coefficients modeling may substantially reduce out-of-sample forecast errors, similar to reductions obtained in several earlier empirical applications of the stochastic coefficients models listed in table 1.

[14]The arbitrariness of these estimates is less harmful in terms of the accuracy of forecasts they lead to than the arbitrariness of prior distributions.

The results in table 1 generally turned out favorably to the stochastic coefficients models because the minimum average mean square error predictors corresponding to these models are evaluated at their respective minimum root mean square forecast error estimates of unknown parameters. These results cannot be reproduced using any arbitrary *a priori* values of parameters. This statement not only elaborates upon footnote 14 but also explains why Alexander and Thomas (*3*) and Wolff (*58*) find that the forecasts of exchange rates generated by the Kalman filter with *a priori* values of parameters are poor relative to the forecasts of random walk models.[15]

We used equation 28 to estimate an agricultural investment model (see *10* for a complete discussion of the model). Investment is assumed to be generated by a linear version of the flexible accelerator where:

$$K_t - K_{t-1} = b_t + b_{wt}W_t + b_{ut}U_t + B_tK_{t-1}, \qquad (29)$$

and W is the ratio of input to output price, K is the capital stock, and U is the implicit rental rate of capital.

We compared the usefulness of the stochastic coefficients specification's forecast accuracy with six other models in five-period out-of-sample tests. Although the six alternatives do not exhaust all possible models, they help evaluate the predictive capability of the stochastic coefficients investment model.

One of the six models is the fixed coefficient analogue of the stochastic coefficients investment model. Net investment is regressed on a constant, an input/output price ratio, a rental rate, and lagged capital stock. Two other models are variants of the fixed coefficients model. One model includes net farm income (income) as a regressor, the other includes a time trend (time). The fourth model is a fixed coefficient, nonlinear flexible accelerator, where the adjustment parameter is a function of the ratio of input to output prices and the rental rate. The final two models are atheoretical. Investment is assumed to be a stochastic process following both a first-order autoregressive AR(1) process and a second-order autoregressive AR(2) process.

Table 2 shows that the stochastic coefficients model is the superior predictor. However, an unambiguous indicator of forecast accuracy does not exist. Each indicator has its own risk function. For example, a mean absolute error criterion is based on an absolute deviation loss function, while a mean square error criterion is based on a quadratic loss function. Therefore, different analysts may prefer different models, depending on their assumed loss function. Considering a wide variety

[15]For examples of the use of stochastic coefficients models in a policy simulation framework, see (*9, 11, 14*).

Table 1—Out-of-sample root mean square forecast errors of stochastic coefficients and fixed coefficients estimators[1]

Source	Dependent variable	Fitting period[2]	Forecast period	Stochastic coefficients	Fixed coefficients[3]	Random walk[4]	Improvement over best alternative
							Percent
Conway, Hallahan, Stillman, and Prentice (1987)	Beef retail price (U.S.)	1968:Q1–1979:Q4	1980:Q1–1983:Q4	7.29	14.00	—	48
	Pork retail price (U.S.)	1968:Q1–1979:Q4	1980:Q1–1983:Q4	5.83	2.91	—	−100
	Broiler retail price (U.S.)	1968:Q3 1979:Q4	1980:Q1–1983:Q4	3.78	6.02	—	37
Conway and Gill (1987)	Fixed weight GNP inflation rate (U.S.)	1960:Q1 1980:Q4	1981:Q1 1984:Q4	.200	.395	—	49
LeBlanc, Kitchen, and Conway (1988)	Exchange rate U.S.$/Canada $	1975:9–1985:2	1985:3–1985:7	.036	.048	0.049	25
Swamy, Kennickell, and von zur Muehlen (1986)	M1 aggregate (U.S.)	1960:Q1–1982:Q2	1982:Q3–1985:Q2	4.644	19.804	—	77
Swamy and Tavlas (1989)	Monetary base (Australia)	1967:Q1–1984:Q3	1984:Q4–1985:Q4	.562[5]	.534	1.767	−5
Swamy and Tavlas (1989)	Monetary base (Australia)	1967:Q1–1984:Q3	1986:Q1–1987:Q2	.883[5]	.960	.978	8
Swamy and Tavlas (1989)	M1 aggregate (Australia)	1967:Q1–1984:Q3	1984:Q4–1985:Q4	1.922[5]	2.109	2.716	9
Swamy and Tavlas (1989)	M1 aggregate (Australia)	1967:Q1–1984:Q3	1986:Q1–1987:Q2	.939[5]	1.159	1.651	19
Swamy and Tavlas (1989)	M3 aggregate (Australia)	1967:Q1–1984:Q3	1984:Q4–1985:Q4	.659[5]	1.173	2.706	44
Swamy and Tavlas (1989)	M3 aggregate (Australia)	1967:Q1–1984:Q3	1986:Q1–1987:Q2	.774[5]	1.555	1.137	32
Swamy, Kolluri, and Singamsetti (1988)	Treasury bill rate (U.S.)	1960:Q1–1983:Q4	1984:Q1–1986:Q4	.411	.585	.658	30
Swamy and Schinasi (1986)	Stock prices	1900–73	1974–83	.680	.877[6]	1.801	22
Schinasi and Swamy (1987)	Exchange rate dollar/pound	1973:3–1980:3	1980:4–1981:6	2.170	3.540	3.030	28
	dollar/yen	1973:3–1980:3	1980:4–1981:6	3.270	4.030	3.960	17
	dollar/deutschmark	1973:3–1980:3	1980:4–1981:6	2.170	2.560	3.690	15
Schinasi and Swamy (1988)	G-10 weighted average dollar	1975:1–1982:12	1983:1–1984:12	2.009	2.181	2.056	2
Schinasi and Swamy (1988)	G-10 weighted average dollar	1976:1–1983:12	1984:1–1985:12	2.618	2.715	2.873	4
Schinasi and Swamy (1988)	G-10 weighted average dollar	1973:1–1984:12	1985:1–1986:12	2.311	2.315	2.712	0

[1]Square root of an average of sum of squared deviations multiplied by 100.
[2]Numbers shown after years signify either quarters (Q) or specific months.
[3]Forecasts based on the best predicting estimates.
[4]$y_t = y_{t-1}$ + white noise.
[5]Forecasts of the same variable for these two different forecast periods are based on identical parameter estimates.
[6]Sequential forecasts.

Table 2—Out-of-sample net investment forecast, 1981-85

Year	Actual	Stochastic coefficients	Fixed coefficients	Income	Time	Flexible accelerator	AR1	AR2
				Million dollars (1972)				
1981	−993	−385	354	623	602	308	610	566
1982	−2,017	−1,169	321	−359	987	120	543	500
1983	−1,962	−1,369	343	−818	1,349	135	502	472
1984	−1,815	−1,359	449	614	1,764	157	478	460
1985	−2,104	−1,845	33	198	1,580	169	463	460

of forecast and other criteria, including goodness of fit and tracking measures, is preferable.

Table 2 presents each model's forecasts for 1981-85. The forecast statistics, based on years with dramatic declines in agricultural investment, provide an excellent test of forecast accuracy. The absolute error shows that the stochastic coefficients model dominates the fixed coefficients models each year. After missing the actual value by a relatively wide margin in 1982 ($849 million), the stochastic coefficients' forecast improves through 1985, where the absolute error is $258 million. The evaluation statistics for each model are mean absolute error (MAE), mean absolute percentage error (MAPE), root mean square error (RMSE), and Theil's U2 coefficient. Table 3 shows that the stochastic coefficients model is the most accurate out-of-sample forecaster. The mean absolute error statistic (MAE) is representative of the stochastic coefficients' dominance over its competitors. The nearest competitor, the flexible accelerator model, is more than three times greater in MAE. The stochastic coefficients model outperforms the other six models for nearly any sensible risk function.

Conclusions

By accepting that the aim of inference is to generate predictions for future observables, we can see that the problem of comparing alternative model specifications is resolved by comparing the accuracy of predictions the models generate and choosing the model that predicts best. Experience with such comparisons suggests that allowing all coefficients in an economic relationship to vary over time may contribute to improved forecasts. The economics literature has long recognized that slopes of economic relationships may not be constant through time because of aggregation effects and policy changes. Therefore, the assumption of time-varying coefficients cannot be so easily dismissed on the grounds that, when coefficients vary, "the concept of seasonal adjustment can become rather confused" (*21,* p. 1,014), or that increasing the complexity of the models used to generate predictions does not necessarily lead to better predictions.

Table 3—Forecast evaluation statistics[1]

Model	MAE	MAPE	RMSE	Theil's U2
Stochastic coefficients	533	34	1.89	0.89
Fixed coefficients	2,297	133	2.17	1.03
Fixed coefficients with income	2,269	131	2.17	1.02
Fixed coefficients with time	2,078	119	2.15	1.02
Flexible accelerator	1,829	109	2.13	1.00
AR1	3,034	170	2.24	1.06
AR2	1,956	112	2.14	1.01

[1]Mean value for net investment during 1981-85 is $1.78 billion.

Swamy and Tinsley's minimum root mean square forecast error estimates will also be useful in assessing Bayesian prior distributions (*48*). If these estimates imply a distribution for the coefficients, which is more general than a Bayesian prior distribution implied by our prior beliefs, then such prior distributions are incapable of producing accurate forecasts.

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Agricultural and Rural Data Paradigms

Robert F. Boxley

Abstract. *Numerous observers have charged U.S. agricultural data systems with conceptual obsolescence. Although some modernization has occurred, particularly in providing information about commercial farms, agricultural and rural data bases have failed to keep up with production and structural changes in the sector and in rural America. I propose a production paradigm for the food and fiber system based on the concept of a primary agricultural producer. Among other attributes, the concept of a primary agricultural producer provides opportunities to redirect existing surveys, possibly including the Census of Agriculture, to the collection of broader rural or natural resource data sets. Before such a redirection can be made, however, a new paradigm of rural people and resources is needed.*

Keywords. *Agricultural data, rural data, economic surveys, information systems.*

Concerns continue to be raised about the adequacy of U.S. agricultural data systems. Many traditional concepts about farms and rural areas are obsolete, yet demands continue for information to manage and evaluate agricultural production, credit, conservation, and rural development programs. The emergence of a bimodal distribution of farms, in particular, has hampered existing data systems from encompassing the full domain of contemporary agricultural and rural concerns. Agricultural policy primarily focuses on mid- to large-scale farms, but these operations increasingly resemble business establishments in other sectors of the U.S. economy. If agricultural policy continues to emphasize a market orientation, as seems likely, the need for more information on farm financial and production characteristics than for other businesses seems open to question.

Small farms are not now primary targets of agricultural policy because they do not contribute significantly to aggregate agricultural output nor are they representative of rural people and businesses. Demands for information about local areas, rural people, and rural economies continue apace, but they cannot be adequately met with most production-oriented information sets.

The common response to perceived information gaps has been to call for yet more surveys and broader data coverage. Bonnen, in particular, has consistently championed the goal of a complete economic paradigm of the food and fiber system. I perceive, however, a growing divergence between the idealized data systems described by Bonnen and others and the complexity of American agriculture and rural society. I question the assumptions that more data about traditional concepts are necessarily desirable, or that a single data paradigm is a reasonable approach to meeting agricultural and rural data needs. Today's budget deficits will surely limit increased funding and political support for expanding Federal agricultural data systems. The pursuit of an all-encompassing data system may obscure gains achieved in improving the precision and focus of current systems.

Consider two paradigms for agricultural and rural data: one for primary food and fiber production and the other for rural people and resources. The production paradigm, I suggest, can be built on the Farm Costs and Returns Survey (FCRS), a joint survey of the National Agricultural Statistics Service (NASS) and the Economic Research Service (ERS). By adopting an FCRS-based data system as the paradigm for the primary stage of food and fiber production, we can redirect resources now devoted to other NASS surveys and the Census of Agriculture to a separate rural data paradigm.

My perspective on agricultural data is that of Federal agricultural policy needs. This perspective does not deny State, local government, and private interests in small-area agricultural data. However, the type of data needed by these entities differs from that required for Federal policymaking, and, I believe, can be furnished more expeditiously as part of a rural data paradigm.

Problems in Current Data Systems

Numerous observers believe the U.S. farm data system to be inadequate. Contributors to this assessment include a special Committee on Economic Statistics of the American Agricultural Economic Association (AAEA) *(3)*, symposia presenters at AAEA annual meetings in 1972 *(12, 17, 27)* and 1987 *(10, 22, 24)*, an AAEA presidential address *(9)*, and authors of numerous book and journal articles (see, for example, *4, 7, 8, 11)*.[1]

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[1]Italicized numbers in parentheses cite sources listed in the References at the end of this article.

The AAEA Committee on Economic Statistics concluded in 1972 that agricultural data systems were "in deep trouble." The committee saw data demands outrunning the profession's investment in data development and the systems' conceptual foundations crumbling. At a symposium marking the 15th anniversary of the committee report, participants concurred that the promise implicit in the report's title of "New Directions and Opportunities" had gone unrealized *(21)*. Bonnen, a chief architect of the report, concluded that data systems remained about where they were in 1972, with progress in some areas offset by changes in agriculture and the policy environment and by continuing erosion of data system capabilities. Bonnen emphasized:

- A declining ability of statistical aggregates to represent an increasingly heterogeneous sector,
- A rising respondent fatigue from repeated surveys of a shrinking universe,
- Declining professional commitment to the empiric,
- Failure to develop a food and fiber sector paradigm consistent with the shift from a demographic to economic Census of Agriculture,
- Failure to recognize structural issues arising out of the progressive concentration of farm production and marketing, and
- Failure to develop a clear statement of the economics of public information.

Stanton noted: "While [the 1972] report got more attention than most presented at our annual meetings, the groundswell of support that followed from our profession could be likened to an almost unnoticed ripple in a turbulent sea." Stanton identified nine areas for improvement, beginning with "a new definition and classification of farms."

Schertz was critical of the emphasis placed on farm commodity data to the "detriment of information on the 80 to 90 percent of people in rural America who are not farm operators." Schertz cited the continued inattention to the underemployment and waste of rural human resources, the persistence of the "one farm—one farmer—one household" myth, and the limited professional recognition given to data work by agricultural economists.

Not all of the symposium's assessment of current data systems was negative. Both Bonnen and Stanton cited improvements in the national income accounts and the development of the FCRS, which Bonnen characterized as "a good example of how you have to run fast just to stand still."

Toward a Production Paradigm for Agriculture

The census of agriculture currently defines a farm as any place from which $1,000 or more of agricultural products were sold or normally would be sold during the census year. Some analysts propose changing the $1,000 threshold, perhaps indexing it for inflation.[2] Other analysts use *ad hoc* farm definitions based on value of sales criteria of $20,000, $40,000, or more, limiting the relevant universe in some cases to well under 1 million operators.

The 1972 committee report declared: "It is simply no longer possible to use the farm as the *basic unit of observation*. . . we will continue to need to construct statistics that say something about physical farms or firms of various sorts, but the farm or firm as the *basic unit of observation* from which all food and fiber statistics are constructed is conceptually obsolete." The committee did not, however, provide many hints about what a new "basic unit of observation" might resemble, and the literature since the 1972 report continues to equivocate on what should be measured. Bonnen argues for a more comprehensive economic paradigm, integrating input suppliers and secondary processors in a systems approach *(7)*. Carlin and Handy proposed an establishment concept, focusing on detailed economic accounts for the entire food and fiber industry, including input suppliers and output processors *(12)*. Others argue for retaining a farm concept but including more expansive measures of farm and farm-related households, such as multiple household entrepreneurial arrangements *(1, 23)*. Stanton would include other economic actors, particularly farmworkers and landlords, who provide resources to agriculture *(24)*.

A common problem with these suggestions is that they would require more information, from more respondents, than at present. Ahearn and Jensen, Schertz, and Stanton, in particular, would require extensive financial and demographic information from a broadened universe of respondents *(1, 23, 24)*.

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[2]The AAEA Committee observed: "The distinction between the definition and the concept of a farm makes efforts to decide whether a farm should begin at $2,500 or $5,000 (or any other level) of gross income intellectually futile. Searching for the *right* definition of a concept such as a farm, when the concept itself is obsolete, is intellectually a bootless enterprise" *(3)*.

Bonnen and Carlin and Handy would substantially expand reporting by firms who supply agricultural inputs and process or distribute food and fiber.

I propose, instead, to focus agricultural production and financial data collection more narrowly on "primary agricultural producers." I define a primary agricultural producer as any business entity producing, or capable of producing in a normal year, nontrivial quantities of agricultural commodities. I use "primary" to designate the first level or stage of production, analogous to the farming establishment component of Carlin and Handy's tableau. A nontrivial quantity of output is any production above a specified threshold level for a given commodity or set of commodities. Producers would be classified according to commodity specialization based on the current standard industrial classification (SIC) system.[3]

The intent of these definitions is to focus data collection on mid- to large-scale producers, or those making economically significant contributions to agricultural output. The production threshold would be set at the increment of production accounting for the *n*th percentage of cumulative commodity output among all producers, ranked from large to small, thus excluding from the universe units producing quantities of output that are insignificant from an aggregate production perspective. A target of 99-percent coverage of total value of agricultural output in 1982, as an example, would have allowed a cutoff point of about $5,000 in sales (1.4 million farms). A 97-percent coverage target (approximately $10,000 in sales) would have reduced the number of farms enumerated from 2.24 million to 1.14 million. A target of 95-percent coverage for some commodities could require enumeration of as few as a quarter or a third of all producers (table 1). Target coverage levels should be determined on benefit-cost principles by recognizing the low marginal utility of production or financial data from the smallest firms, and the potential marginal cost of enumerating many small producers.[4]

[3]For example, "cash grain producers" would identify (a) all individuals or organizations that (b) grow and harvest for sale, temporary storage, or further processing, (c) one or more grain crops classified under SIC category 011; and (d) who, from a ranking of largest to smallest producers, produce or are capable of producing, say, 99 percent of all cash grain crops for a given year.

[4]Expected cost savings from excluding the smallest producers may depend on the type of survey. The FCRS consists of both a list frame of farm operators and an area frame of rural land. In the area frame sample, all residents within a geographic area are typically contacted. In this case, the marginal cost of collecting data for otherwise ineligible respondents amounts to only interview time and processing costs. For list frame sampling, the problem is knowing the total universe so that a threshold can be determined. I assume, however, that a combination of information from area frames, crop surveys, and marketing reports would allow determining the threshold level and identifying producers for the list frame.

Table 1—Firms accounting for approximately 95 percent of total sales, selected commodities, 1982

Lower limit of sales class and commodity	Proportion of: Sales	Proportion of: Farms
	Percent	
$100,000:		
Poultry and poultry products	94.0	25.5
$40,000:		
Cotton and cottonseed	95.3	60.9
Vegetables	94.8	34.9
Horticultural specialties	94.8	34.7
Dairy	94.8	72.1
$20,000:		
Grains	94.6	62.1
$5,000:		
Tobacco	95.5	69.3

Source: 1982 Census of Agriculture, Vol. 1, part 51, table 49.

Tradeoffs between enumerating the smallest producers and reducing variances of data items within the survey universe could be made more explicitly than at present. By specifying the universe in terms of output percentages, the definition of a producer remains comparable over time, regardless of variation in aggregate commodity production, commodity prices, or general price levels.

"Primary agricultural producer" is intended to be an economic concept. Only production or financial data germane to analyses of the organizational entity, which could be any person, firm, organization, or separable organizational component, would be collected. Measures that confuse the boundaries between households and production units, or that intermingle personal and firm attributes would be dropped unless required for a specific analytical purpose.

Geographic coverage would be determined by the number and spatial distribution of producers and desired precision levels for the enumerated items. Assuming current FCRS funding levels, coverage goals might extend to some, but not all, crop reporting districts in major agricultural States.

Information about primary producers would be collected annually by the FCRS (renamed the "Producer Cost and Returns Survey," or PCRS). Present coverage by the FCRS, in aggregate, is capable of meeting a relatively high threshold. The 1987 survey was based on 24,000 survey contacts which yielded a 73-percent completion rate *(19).* The 1987 survey accounted for 77 percent of the total number of farms officially

reported by USDA, with most of the difference between official farm numbers and FCRS estimates attributed to an undercount of farms with less than $10,000 in annual gross sales.[5]

Role for the Census of Agriculture

What becomes of the census of agriculture with the PCRS? One option would be to continue the census using current definitions and procedures. This would assure county-level coverage and continuity with historical data but would also perpetuate the notion of a "farm" inconsistent with the concept of a producer.[6] Another would be to align the census with the concept and universe of the PCRS. Although there is substantial overlap, the current FCRS mainly measures costs and other financial aspects of farm production, while the census primarily measures physical assets, production inputs, and outputs. Because production processes tend to be more stable than financial processes, the combination of an annual PCRS and a periodic census survey of production parameters could yield a more comprehensive data system for agricultural producers than either survey alone. Still, I question if focusing both the PCRS and census on agricultural producers is the best use of data resources.

The county-level detail available from the census of agriculture is valuable to many users. However, county data are increasingly compromised by efforts to avoid disclosure; the 1982 Census encountered disclosure problems extending to a number of data items at the State level *(6)*. The continued usefulness of county data, especially for aggregate economic or policy analysis, may be questioned. Is statistically acceptable coverage of all counties with any agricultural activity, no matter how limited, cost effective from a Federal perspective?

Many users view the census of agriculture as an indispensable source of economic and demographic information about rural people. A specific concern for these users likely will be that a PCRS, by deliberately excluding small producers and by treating farming solely as an economic enterprise, cannot fully reflect the unique cultural and social institutions surrounding agriculture. To retain the census of agriculture on these grounds, however, would seem to require more harmony in defining "rural" and "farm" than now exists. Considering the minority position of farmers in most areas of the Nation (27 percent of the U.S. population resides in rural areas but only 8 percent of the rural population actually resides on farms), relying on census of agriculture data to measure social or environmental phenomena gives a distorted view of rural economies. A $1,000 sales threshold may be entirely arbitrary in many areas in distinguishing farms from other rural households that also contribute to rural economies and serve as "custodians of rural landscapes, communities, cultures, institutions, and values" *(14)*.

A Rural Resource Paradigm?

The nature of a rural paradigm is much less clear than that of a PCRS *(7, 8)*. The 1972 AAEA committee report concluded: "The very notion of rural needs to be evaluated in a conceptual sense." In his review of the committee report, Lee commented:

> One reaction is that the committee report has more to say that is useful about food and fiber industry statistics than about people and social statistics. Perhaps this merely reflects the fact that we have a system of industry statistics that we criticize and get specific about. We do not even have that to start with on the people side. Furthermore, the latter subject is probably more complex and certainly more diverse, thereby increasing the difficulty of conceptualization and of a systematic approach *(15)*.

A 1987 ERS report to the U.S. Senate Appropriations Committee noted the difficulty of defining rural issues in a world where the rural economy has become an integral part of national and global economies *(25)*.

The primary information needs about rural people and rural activities would seem to be the same as for urban areas notwithstanding the conceptual difficulties of defining rural. A rural paradigm, therefore, might build on existing demographic, business, and local government surveys and censuses of the Department of Commerce. Funding for the current census of agriculture could be redirected to supplement other annual or special census surveys in rural areas, or to add additional questions on the long-form decennial population census about issues of relevance to both urban and rural citizens, such as commuting patterns or multiple jobs.

[5]For the 1985 FCRS, the coefficient of variation for the national estimate of 1.55 million farms was 1.93 percent *(17)*. Regional coefficients of variation for farm numbers ranged from 3.25 percent in the Corn Belt to 8.24 percent in the Pacific States. Coefficients of variation for less frequently encountered items in an FCRS can be quite large, and State-level reliability for minor items may be expensive to obtain.

[6]Allen and Pautler argue that the census provides a reliability check for the more specialized NASS surveys, that data quality is an implicit benefit of duplication, and that having both sources "stimulates reviews of statistical procedures" *(2)*.

"Rural" suggests two identifying characteristics: geographic space and ties to natural resources. Income, employment, health, education, and social concerns of rural people largely parallel those of urban dwellers. These concerns may be compounded by factors of distance and population density. Economic dependence on natural resources still characterizes many rural areas, although relationships may be weakening *(5)*. And, frequently, use of natural resources in rural areas is central to broad environmental degradation or protection concerns. Thus, the key to a rural data paradigm may be better coordination of existing demographic, resource, and institutional information. A promising development is geographic reference systems *(20)*. For the 1990 Census of Population and Housing, the Bureau of the Census and the U.S. Geological Survey are implementing a "Topographically Integrated Geographic Encoding and Referencing" (TIGER) system, which promises to provide a useful framework for integrating spatial measures with census information on population, housing, income, and resource use *(16)*.

USDA is a logical focus for natural resource data collection related to rural areas. Present legislative mandates to USDA include Section 302 of the Rural Development Act of 1972, Section 5(A) of the Soil and Water Resources Conservation Act of 1977, and Section 3 of the Forest and Rangeland Renewable Resources Planning Act of 1974 *(13)*.[7] Responsibilities for carrying out this legislation currently fall to the Soil Conservation Service and Forest Service. Data collection might usefully be transferred to National Agricultural Statistics Service as a building block for a comprehensive rural resource data base. NASS experience with area frame sampling would be advantageous for resource-based surveys. NASS has also entered into a long-term agreement with the National Aeronautics and Space Administration to develop further data applications of remote-sensing technology.

Conclusions

The economics and statistics professions have largely failed to address charges of conceptual obsolescence raised 16 years ago by the AAEA Committee on Economic Statistics. Present agricultural data systems convey the appearance of duplication and waste of survey resources. For these reasons, revision in agricultural and rural resource data systems should command high priority. Perhaps the greatest strength of the collaboration between NASS and ERS has been their joint assumption of responsibility in developing the FCRS. The design and administration of a PCRS seems a reasonably straightforward extension of procedures already established for the FCRS. Prospects for a rural data paradigm seem much less certain, but a framework for such a paradigm exists in current efforts to integrate population and resource data within a geographic reference system.

[7]Section 302 authorizes a continuing land inventory and monitoring program including identification of prime farmland, studies and surveys of erosion and sediment damage, flood plain identification and use, land use changes and trends, and environmental degradation. Section 5(A) directs the Secretary of Agriculture to conduct a continuing appraisal of soil, water, and related resources, and also authorizes special-purpose inventories. Section 3 directs the Secretary to conduct comprehensive surveys and analyses of present and prospective conditions of renewable resources, forest, and rangeland.

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Dynamic Factor Demands Using Intertemporal Duality

Bruce A. Larson

Abstract. *Intertemporal duality can be used for empirical research to derive a system of optimal choice functions (dynamic factor demands and output supplies) consistent with an explicit dynamic optimization framework. While the literature on intertemporal duality focuses on infinite-horizon autonomous problems, many applied problems cannot be analyzed within this framework. This article uses intertemporal duality to specify a system of optimal choice functions for a broader and less restrictive set of intertemporal planning problems.*

Keywords. *Intertemporal duality, dynamic factor demands, Hamilton-Jacobi equation, dynamic optimization.*

Agricultural production is inherently uncertain and dynamic. Lags exist between variable input use and output realization, and biological and manufactured assets are managed over time. To analyze such dynamic production processes, agricultural economists have continually searched for improved empirical methods to analyze shortrun and longrun decisions and explain response to price, policy, and technical changes.

Optimal resource allocation and production has become a common issue analyzed at the theoretical level (*1, 2, 6, 13, 15, 19, 30*).[1] Given the profound influence of static duality theory on theoretical and empirical investigations of firm and consumer behavior (*4, 5, 8, 9, 12, 21, 31*), literature on duality relationships for dynamic optimization problems has also grown (*3, 10, 14, 17, 26*). For certain dynamic optimization problems, duality relationships provide a convenient method for modeling optimal choice functions (output supplies, consumption and factor demands, investment demands).

For example, in the context of an adjustment-cost model of the firm, Epstein develops the duality between a production function and the maximized present value of profits, which is then exploited to derive the firm's system of investment and factor demands via the dynamic analogue of Hotelling's Lemma (*17*). Cooper and McLaren provide similar results in the context of consumer theory (*14*). Chambers and Lopez generalize Epstein's problem and extend their results to a dynamic model of the financially constrained farm household and to a model of optimal fisheries management (*10*). Taylor and Monson, and Vasavada and Chambers use this approach to study investment in U.S. agriculture (*33, 36*). The dynamic duality approach could be applied to many intertemporal planning problems where assets are managed over time, including animal husbandry, mining industries, and forestry or agro-forestry production.

Cooper and McLaren distinguish three types of duality relationships for dynamic optimization problems: (1) atemporal duality, which refers to the relationship between instantaneous functions at one point in time, such as between utility and indirect utility functions; (2) temporal duality, which refers to the relationship between the present values of sequences of functions (optimal value functions), such as between the maximized present value of utility and the minimized present value of expenditures over a given time horizon; and (3) intertemporal duality, which refers to the relationship between an instantaneous function and a corresponding optimal value function. While atemporal and temporal duality are essentially equivalent to that surveyed by Diewert (*14*), intertemporal duality provides a convenient method for modeling optimal choice functions.

Intertemporal duality is based on the Hamilton-Jacobi equation, which links the optimal value function to the instantaneous function via a static optimization problem. The dynamic analogues of Hotelling's Lemma or Roy's Identity can be found by applying the envelope theorem to the Hamilton-Jacobi equation, which can be written in various forms depending upon the structure of the intertemporal problem. While the literature focuses on infinite-horizon autonomous problems, there are many applied issues that cannot be analyzed within this framework.[2] Therefore, intertemporal duality could be applied to a broader range of planning problems.

Larson is an economist with the Resources and Technology Division, ERS. He appreciates the helpful comments of Kathleen Segerson, Howard Leathers, Roger Conway, and John McClelland on this article.

[1]Italicized numbers in parentheses cite sources listed in the References at the end of this article.

[2]Optimal control problems are said to be autonomous when the current-value Hamiltonian is not an explicit function of time, in which case the solution to the problem involves solving an autonomous system of ordinary differential equations. For example, consider an ordinary differential equation of the form $dx/dt = g(x,u,t)$, $x(t_0) = x_0$, where x is a state variable, t is time, and u is a control variable. This system is said to be stationary if g is not an explicit function of time ($dx/dt = g(x,u)$), is free when $u=0$ for all $t \geq t_0$, and is autonomous when it is stationary and free (*7*, p. 448).

The main objectives of this article are to discuss the various types of applied problems that could be analyzed with the intertemporal duality approach, and to provide convenient forms of the Hamilton-Jacobi equation for these problems. The results of this article can be used as the foundation for further theoretical and empirical applications of the intertemporal duality approach.[3] I show in the autonomous case how dynamic factor demands for infinite-horizon autonomous problems can be determined. Using an adjustment-cost model of the firm based on the method outlined for the autonomous case, I then develop a convenient form of the Hamilton-Jacobi equation for infinite-horizon nonautonomous problems and provide the appropriate analogue of Hotelling's Lemma.

Duality for Finite-Horizon Problems

Consider the following finite-horizon nonautonomous problem A:

$$J(x_o,t_o t_1,b) = \underset{u \in U}{\text{Max}} \int_{t_o}^{t_1} f(x,u,t,b)dt \tag{1}$$

$$\text{s.t.} \quad dx/dt = g(x,u,t,b);\ x(t_o) = x_o,\ x(t_1) \text{ free,}$$

where t is time, t_o is initial time, t_1 is terminal time; u(t) are control variables; x(t) are state variables; x_o are initial states; b are constant parameters, such as prices, taxes, or other policy variables; f is the intermediate function; dx/dt = g are the state equations; and U is the control set. Given a set of conditions to ensure a solution to problem A, the optimal value function $J(x_o,t_o,t_1,b)$ is defined as the optimal value of the objective functional for the problem with initial state x_o that begins at t_o and ends at t_1, given the parameters b.

Chavas and others used a variation of problem A to model the present net value of a biological asset (hogs)(*12*). In that case, u is variable inputs, x is the state of the asset (weight), b is a vector of prices and the discount rate, f(x,u,t,b) is the net revenue of flow products obtained from the asset, dx/dt = g is a biological growth function, t_o is time of purchase, and t_1 is time of sale. Net revenues are an explicit function of t if the flow of outputs varies with the age of the asset. While Chavas and others (*12*) considered the case of animal replacement, the asset could just as well be a perennial crop or a tree from which products are obtained (milk, coffee, firewood, palm oil, gum arabic, oranges).

Finite-horizon investment problems also occur when a farmer leases a farm for a fixed period of time. In that case, u represents variable inputs, x represents the farm capital stock, and net investment dx/dt equals gross investment less depreciation.

In general, the maximum principle could be used to solve for the optimal choices of u in problem A over the period t_o to t_1, but in practice an analytical solution is usually difficult to obtain. However, at any initial time $t_o \leq t_1$, it is well known that the Hamilton-Jacobi equation for problem A takes the general form (*20, 22*):

$$-\frac{\partial J}{\partial t_o} = \underset{u}{\text{Max}} \Big[f(x_o,u,t_o,b) + \frac{\partial J}{\partial x_o}(x_o,t_o,t_1,b)g(x_o,u,t_o,b)\Big] \tag{2}$$

The significance of the Hamilton-Jacobi equation is that the maximizing u in equation 2, $u^* = u^*(x_o,t_o,t_1,b)$, are the optimal controls to problem A for the initial time t_o. Differentiating equation 2 with respect to b and using the envelope theorem provides the dynamic analogue of Hotelling's Lemma:

$$-\frac{\partial^2 J}{\partial t_o \partial b} = \frac{\partial f}{\partial b} + \frac{\partial^2 J}{\partial x_o \partial b} g + \frac{\partial J}{\partial x_o}\frac{\partial g}{\partial b} \tag{3}$$

Given forms for J, f, and g, and an error structure, u* can be, in principle, estimated from equation 3 by using simultaneous equation techniques for implicit functions (*18*), although certain functional forms allow more direct estimation procedures. Thus, intertemporal duality allows the optimal choice functions, $u^* = u(t_o,t_1,x_o,b)$, to be derived from J, f, and g without the need to solve problem A. Because equation 3 is a nonlinear function of the variables u and x and the parameters b, further assumptions on the functions f and g can improve the empirical tractability of the system.[4]

Equation 2 is the general form of the Hamilton-Jacobi equation. In the remainder of this article, I explore some simple variations of problem A that allow the Hamilton-Jacobi equation and, therefore, the

[3]Although the focus of this article is intertemporal duality, one can also use other approaches to derive systems of dynamic factor demands. For example, Pindyck and Rotemberg (*29*) specify a discrete-time infinite-horizon problem, and then make use of static duality relationships to estimate a static cost function, an energy cost share equation (the variable input), and Euler equations for capital and labor (the quasi-fixed inputs). Lopez (*23*) follows a similar approach in a continuous-time model.

[4]For example, as in static duality, equation 3 is much simplified when the objective functional is linear in b and the state equations are not a function of b.

analogues of Hotelling's Lemma to be written more simply. For example, consider the following infinite-horizon autonomous problem B:

$$J(x_o,t_o t_1,b) = \underset{u}{\text{Max}} \int_{t_o}^{t_1} f(x,u,b)dt \tag{4}$$

$$\text{s.t.} \quad dx/dt = g(x,u,b);\ x(t_o) = x_o,\ x(t_1) \text{ free}$$

Because time is not an explicit argument in f and g (problem B is autonomous), the Hamiltonian evaluated at the optimizing u is constant for all t, $t_o \leq t \leq t_1$ (*20*). Therefore, the Hamilton-Jacobi equation can be written as:

$$-\frac{\partial J}{\partial t_o} = H^*, \tag{5}$$

where H* is the Hamiltonian for problem B evaluated at the optimal u. Integrating both sides with respect to initial time t_o and evaluating over the interval t_o to t_1 yields:

$$\frac{J(t_o,x_o,t_1,b) - J(t_1,x_o,t_1,b)}{(t_1 - t_o)} = H^* \tag{6}$$

But, $J(t_1,x_o,t_1,b) = 0$ because it is defined as the optimal value of the objective functional for the problem beginning and ending at time t_1, and there is no scrap value. The term $1/(t_1 - t_o)$ converts the sum J received every t_1–t_o periods into a constant flow every period. Depending upon the situation, it could be more useful to derive the analogue of Hotelling's Lemma from either equation 5 or 6.

Duality for Infinite-Horizon Problems: The Autonomous Case

The specific form of problem A in much of the literature on duality theory and dynamic factor demands is some variation of problem C:

$$J(t_o k_o,p) = \underset{I \geq 0}{\text{Max}} \int_{t_o}^{\infty} e^{-rt}[f(k,I) - pk]dt \tag{7}$$

$$\text{s.t.} \quad dx/dt = I - \alpha k;\quad k(t_o) = k_o,$$

where I(t) is investment in capital k(t); p is the rental price of capital normalized with respect to output price; α is the depreciation rate; f is the production function; $dk/dt = I - \alpha k$ is the capital stock equation of motion, and r is the firm's discount rate. J is not written as an explicit function of α and r to reduce notational clutter.

Problem C describes an adjustment-cost model of the firm with static-price expectations (see *17, 24, 27, 35*).[5] However, the structure of problem C is also similar to: (1) models of the extractive firm, where k is the mineral stock, I is the rate of extraction, and dk/dt = –I (see *11*); (2) farm-level models of soil conservation, where k is topsoil depth, I is erosion, s is natural regeneration, and dk/dt = s – I (see *25*); and (3) forest-harvesting models, where k is tree biomass, g(k) is the tree-growth function, I is the harvest rate, and dk/dt = g(k) – I (see *13*).

There are two important distinctions between problem C and problem A. First, problem C is autonomous in the sense that its current-value Hamiltonian is not an explicit function of time (time enters only through the discount term). And second, problem C has an infinite time-horizon beginning at any time t_o, but discounted to time 0. Given these two conditions, the optimal value function for problem C can be written as:

$$J(t_o k_o,p) = e^{-rt_o} V(k_o,p), \quad \text{where:}$$

$$V(k_o,p) = \underset{I \geq 0}{\max} \int_{t_o}^{\infty} e^{-r(t-t_o)} [f(k,I) - pk]dt \tag{8}$$

$$\text{s.t.} \quad dk/dt = I - \alpha k;\quad k(t_o) = k_o$$

The optimal value function V in equation 8 is not written as an explicit function of t_o because $\partial V/\partial t_o = 0$ (*2, 22*). Since $-\partial J/\partial t_o = rV\exp(-rt_o)$ and $\partial J/\partial k_o = [\partial V/\partial k_o]\,[\exp(-rt_o)]$, the Hamilton-Jacobi equation for an infinite-horizon autonomous problem at an arbitrary time t_o (*22*, pp. 241-2) can be written as:

$$rV(k_o,p) = \underset{I \geq 0}{\text{Max}} \left[f(k_o,I) - pk_o + \frac{\partial V}{\partial k_o}(k_o,p)[I - \alpha k_o]\right] \tag{9}$$

In the literature, initial time is usually considered to be $t_o = 0$, in which case J = V, and the Hamilton-Jacobi equation for problem C can be written as:

$$rJ(k_o,p) = \underset{I \geq 0}{\text{Max}} \left[f(k_o,I) - pk_o + \frac{\partial J}{\partial k_o}(k_o,p)[I - \alpha k_o]\right] \tag{10}$$

[5]The assumption of static expectations implies that the decision unit acts as if all prices will remain constant throughout the planning period. However, if prices change, then the firm resolves the problem at that time. Therefore, only the $t=t_o$ optimal controls are actually observed in practice. Chambers and Lopez (*10*) discuss this assumption more thoroughly, while Taylor (*34*) considers a general problem that includes price uncertainty.

The maximizing investment decisions from 10, $I^*=I(k_o,p)$, are the optimal choices for problem C at $t_o=0$. By the envelope theorem, the derivative of equation 10 with respect to p yields, after rearranging, the intertemporal analogue of Hotelling's Lemma for problem C:

$$I^*(k_o,p) = \left[\frac{\partial^2 J}{\partial k_o \partial p}\right]^{-1} \left[r\frac{\partial J}{\partial p} + k_o\right] + \alpha k_o \qquad (11)$$

Equation 11 provides a simple way to derive systems of investment equations that are consistent with an infinite-horizon autonomous control problem. In contrast to problem A, since the objective functional in problem C is linear in p and the state equation is linear in I (and independent of p), the optimal investment I* can be written only in terms of the indirect objective function J. While the theoretical investment equation 11 is simple to derive, it is potentially nonlinear in variables and parameters and, as a result, may be difficult to estimate. Epstein (*17*) includes variable inputs into the analysis, derives the properties of J, and also discusses the issue of functional forms for J.

Two recent studies that apply the adjustment-cost model of the firm to U.S. agriculture are Taylor and Monson, and Vasavada and Chambers (*33, 36*). Based on theoretical models similar to problem C, both studies use intertemporal duality to specify dynamic factor demands and output supply.[6] Each study proceeds by specifying a functional form for J, imposing conditions on J to ensure consistent aggregation (which is discussed in *36*), and then deriving the analogues of equation 11 based on the Hamilton-Jacobi equation for infinite-horizon autonomous problems.

Duality for Infinite-Horizon Problems: The Nonautonomous Case

When Vasavada and Chambers (*36*) or Taylor and Monson (*33*) actually estimate the system of net investment equations, they include time trends as exogenous variables to reflect the effect of technical change in agriculture over time. While Vasavada and Chambers add a linear time trend onto equation 11, Taylor and Monson include time explicitly into the optimal value function, but then use the Hamilton-Jacobi equation for an autonomous problem to derive the investment equations.

[6]For example, Taylor and Monson (*33*) consider labor and materials to be variable inputs, while land and capital are considered to be quasi-fixed inputs.

Technical change in the production function provides one case where an infinite-horizon control problem may not be autonomous. For example, Hicks-neutral technical change implies that the firm's production function can be written as $y(t) = f(k,I)A(t)$, where $A(t)$ describes the process of technical change in the production of output y. If the firm knows or expects that technical change will occur over time, then the firm must solve a nonautonomous problem to find its optimal investment choices.

There are many other examples where an infinite-horizon nonautonomous formulation would be appropriate. As suggested for problem A, the objective functional for infinite-horizon problems could depend explicitly on time if output is a flow product (milk) from a biological asset. In problem C, the capital price, p, would also be a function of time if the firm expected prices to rise over time (for example, $p(t) = p(t_o)\exp[m(t-t_o)]$, where m is the expected rate of price increase). The state equation for an infinite-horizon problem could be a function of time if the depreciation rate α depended on an asset's age or if technical change affected the rate of asset depreciation.

Problem D is a simple variation of problem C that incorporates time into the production function to represent expected disembodied technical change:

$$J(k_o t_o,p) = \max_{I \geq 0} \int_{t_o}^{\infty} e^{-rt} [f(k,I,t) - pk]\ dt \qquad (12)$$

$$\text{s.t.} \quad dk/dt = I - \alpha k; \quad k(t_o) = k_o$$

Problem D is a nonautonomous control problem because the firm's production function is an explicit function of time. Therefore, the Hamilton-Jacobi equation can no longer be written as equation 9. Fortunately, a convenient form of the Hamilton-Jacobi equation for problem D can be derived using the same process as followed for problem C. First, define:

$$J(t_o k_o,p) = e^{-rt_o} V(t_o,k_o,p), \text{ where:}$$

$$V(k_o,t_o,p) = \max_{I \geq 0} \int_{t_o}^{\infty} e^{-r(t-t_o)} [f(k,I,t) - pk]\ dt \qquad (13)$$

$$dk/dt = I - \alpha k; \quad k(t_o) = k_o$$

Because equation 13 is a nonautonomous problem, the optimal value function V is an explicit function of initial time t_o. Using the definitions of J and V from equations 12 and 13, which imply that $-\partial J/\partial t_o = [rV - \partial V/\partial t_o][\exp(-rt_o)]$ and $\partial J/\partial k_o = [\partial V/\partial k_o][\exp(-rt_o)]$, the Hamilton-Jacobi equation at t_o can be written as:

$$rV(k_o,t_o,p) = \max_{I \geq 0} \{ f(k_o,I,t_o) - pk_o + \frac{\partial V}{\partial k_o}[I - \alpha k_o] + \frac{\partial V}{\partial t_o} \} \quad (14)$$

By the envelope theorem, the derivative of equation 14 with respect to p yields, after rearranging, the dynamic analogue of Hotelling's Lemma:

$$I^*(k_o,t_o,p) = \left[\frac{\partial^2 V}{\partial k_o \partial p}\right]^{-1} \left[r\frac{\partial V}{\partial p} + k_o - \frac{\partial^2 V}{\partial t_o \partial p}\right] + \alpha k_o \quad (15)$$

Equation 15 shows how to derive the optimal investment choice at t_o for problem D, which is an infinite-horizon nonautonomous control problem. There are two important differences between equations 15 and 11. First, the optimal value function V is an explicit function of initial time for nonautonomous problems. Second, equation 15 has the additional term $\partial^2 V/\partial t_o \partial p$. Since $\partial V/\partial t_o$ is the marginal value of technical change at the initial time in problem D, the term $\partial^2 V/\partial t_o \partial p$ is the change in the marginal value of technical change due to a change in the rental price of capital.

Therefore, if the issue to be studied involves an infinite-horizon nonautonomous problem, the empirical model could be based on a Hamilton-Jacobi equation similar to equation 14, which would allow a time variable to be incorporated into the empirical model in a consistent manner.[7] The approach followed above can be applied to any infinite-horizon nonautonomous problem with discounting.

Conclusions

In this article, the Hamilton-Jacobi equation was derived for four general classes of dynamic optimization problems. The envelope theorem can then be applied to the Hamilton-Jacobi equation to specify systems of optimal choice functions. The output supply function can also be derived from a minimization problem dual to the Hamilton-Jacobi equation (see *17*). While the properties of the optimal value function for infinite-horizon autonomous models such as problem C are well known, further research is needed to identify the usable properties of optimal value functions for the other types of problems. To date, far more empirical studies for each class of problems are needed to determine if intertemporal duality will be as useful to applied researchers as static duality.

[7]Whether time trends are the appropriate way to model technical change is beyond the scope of this paper. However, the possible empirical problems with trend analysis should be considered (*28*). A possible alternative would be to include expected technical change as a capital-augmenting stochastic process (see *32*).

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Contemporaneous Correlation and Modeling Canada's Imports of U.S. Crops

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Abstract. *A multicrop model of Canadian demand for U.S. crops is estimated with Zellner's seemingly unrelated regression (SUR), which corrects for the distortion problem in contemporaneous correlation, and with ordinary least squares (OLS), which ignores the problem. Comparing inference parameters, trade elasticity estimates, and out-of-sample forecast performance of the Canadian import demand model's SUR and OLS versions demonstrates the importance of addressing contemporaneous correlation, even though both estimators are unbiased. This article addresses three shortcomings of the agricultural trade literature: frequent failure to account for easily corrected econometric problems; excessively wide ranges of trade parameter estimates; and frequent failure by researchers to validate models beyond the sample.*

Keywords. *Canadian imports, wheat, corn, contemporaneous correlation, Zellner's seemingly unrelated regression, ordinary least squares, price elasticities of import demand, forecast performance.*

U.S.–Canadian agricultural trade has increased in importance with the 1988 ratification of the North American Accord by the U.S. Congress and the Canadian Parliament. The accord is a trade liberalization pact which will "modify or sweep away a wide range of restrictions on transborder commercial and financial dealings" (*11*, p. 16, *18*).[1] A symposium, "Farm Policy for a Freer Trade World," was held May 4-6, 1988, in Quebec, which brought together hundreds of agricultural trade experts to discuss trade liberalization issues, especially between the United States and Canada. This symposium reflects the heightened professional interest in U.S.–Canadian agricultural trade. This interest should help recall the criticism of the profession for ignoring such a frequently encountered, easily corrected, and performance-distorting econometric problem as contemporaneous correlation (*2, 10*).

I demonstrate how failing to correct for contemporaneous correlation among seemingly unrelated Canadian demands for U.S. cotton, rice, and soybeans (hereafter called the model) influences the model's estimate efficiency (and, hence, inference parameters), point estimates, and forecast accuracy beyond the sample (model performance). Researchers involved with U.S.–Canadian agricultural trade employ inference parameters to analyze policy-pertinent parameter estimates, use parameter estimates to ascertain consequences of policy proposals, and compare forecasts to evaluate policy alternatives. I demonstrate that correcting for contemporaneous correlation greatly influences the Canadian import model's performance.

Researchers should ultimately correct for all supply-side and demand-side econometric problems, such as contemporaneous correlation, serial correlation, and simultaneous equations bias through proper econometric method. This article focuses on the impacts on model performance of a single and specific econometric problem, contemporaneous correlation, confronting U.S.–Canadian flows of the three crops.

Contemporaneous correlation often characterizes sets of economic relationships and occurs when the relationships, despite different sets of explanatory variables, have "disturbances. . .correlated at a given point in time. . .[and] not correlated over time" (*6*, pp. 245-6). Sets of such equations are often called "seemingly unrelated." Contemporaneous correlation arises from omission of variables which are of indirect, rather than direct, relevance to the study. Albeit unbiased, OLS estimates of seemingly unrelated equations are of questionable efficiency because the information inherent in the equations' contemporaneously correlated errors is neglected (*2, 7, 8*). Seemingly unrelated equations are appropriately estimated with Zellner's SUR. Without autocorrelation and lagged endogenous regressors, SUR estimates of seemingly unrelated equations are unbiased, asymptotically consistent, and efficient (*7, 8*). Kmenta (*8*) suggests that SUR estimates have small-sample properties similar to the asymptotic ones.

Forecast errors are for out-of-sample predictions, throughout. Analysis of forecast errors is in terms of absolute value of such errors. A coefficient denotes regression estimates of the coefficient's true value. A standard error estimate denotes the sample estimate of the standard error of the estimated coefficient.

Scenario Design

I estimate Canadian demands for U.S. supplies of cotton, rice, and soybeans with SUR and OLS. SUR corrects for the equations' contemporaneous correlation,

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[1]Italicized numbers in parentheses cite sources listed in the References at the end of this article.

while OLS ignores the problem. I analyze the SUR/OLS differences in standard error estimates to discern the efficiency gains from correcting for contemporaneous correlation. I then analyze the SUR/OLS differences in the equations' own-price elasticities to ascertain how the model's contemporaneous correlation influences trade parameter point estimates. Using the information on the model's contemporaneous correlation enhances, or fails to impede, forecast accuracy in most cases.

Data and the Estimated Model

Basic trade theory posits a nation's import demand for a commodity as an excess demand, that is, the difference between domestic demand and domestic supply. Excess demands thus contain both domestic demand and domestic supply arguments. I formulated the three Canadian demands for U.S. crops as Marshallian demands, without domestic supply-side variables, for two reasons: because Canada imported the three crops almost exclusively from the United States from 1965-82 and because Canada produced little or none of those crops (*1, 14, 15, 16, 17*).

I estimated Canadian demands for U.S. cotton and soybeans with SUR and OLS, using 1965-82 annual data from Agriculture Canada (*1*). Estimated are SUR and OLS versions of a Canadian import demand for U.S. rice by using 1965-82 annual data from the U.S. Department of Agriculture (USDA) (*17*). Observations for 1983, 1984, and 1985 were saved for forecasting.

All prices reflect deflated Canadian dollars. Nominal prices used in conversions to denominations of deflated Canadian dollars mirror calendar year data from the International Monetary Fund (IMF) (*5*) for the U.S. cotton price (10 markets), U.S. rice price at New Orleans, price of Canadian wheat, and price of U.S. soybeans. The real polyester price is included in the Canadian cotton relation based on previous work (*2, 3, 4*). The nominal polyester price published by USDA is converted to deflated Canadian dollars (*12*).

I initially included several region- and event-specific indicator variables in line with previous research (*2, 3, 4*). Taking a unity value for 1971-72 and a zero otherwise, X7172 (table 1) captures the influences of the initial stages of breakdown in the Bretton-Woods system of fixed exchange rates. Following Duffy, I included X78, a variable valued at 1.0 after 1977 and at zero prior to 1978 (see *4*). This variable captures the Organization of Petroleum Exporting Countries' (OPEC) real crude petroleum price increases of the late 1970's.

I included the IMF's (*5*) index of Canadian hourly wages as the Marshallian income variable. This wage variable was deleted from all but the cotton equation because of statistical insignificance. The wage index may be collinear with the real price of crude petroleum because Canada is a major energy producer.

Econometric Estimates

Table 1 shows the SUR and OLS estimates for the Canadian demands for U.S. supplies of cotton, rice, and soybeans. Evidence is insufficient at the 95-percent confidence level to suggest serial correlation.

I included the real crude petroleum price (RLPET) for two reasons. First, the large geographic area covered by the United States and Canada means that transportation and related costs significantly influence the cost of crop imports. Second, RLPET may generate a positive sign, as with the rice and soybean equations, because Canada is a major energy producer. Previous research has employed a real petroleum price variable as a proxy for a region's real income trends (*2*, p. 15). The cotton relation excludes RLPET because sample evidence suggests collinearity with the real price of polyester, a petroleum-based substance.

Efficiency and Improved Inference

Estimating seemingly unrelated equations with OLS ignores contemporaneous correlation and generates inefficient estimates. Estimating these relations with SUR uses such correlations and generates efficient estimates (*7, 8*).

Inference parameters for the coefficient estimates are improved through increased efficiency. Smaller standard errors imply more precise confidence intervals for coefficient estimates and for the trade parameter estimates that certain coefficients imply. Also, increased efficiency through SUR translates into t-values altered from OLS-generated levels, providing clearer indications of the relationships between regressors and the dependent variable.

Table 2 shows the estimated standard errors for SUR- and OLS-generated regression estimates associated with Canadian import demands. Choosing SUR over OLS resulted in evidence that strongly suggests efficiency gains. SUR-estimated standard errors declined from OLS-generated levels for each coefficient in all three equations, as econometric theory would suggest (*8*, pp. 517-25). Yet, the degree of such gains, and their degree of improved inference reliability, is a study-specific gain which is very important to researchers of U.S.–Canadian agricultural trade. Table 2's SUR-generated estimates in standard errors declined from

Table 1—Econometric estimates of Canadian imports of U.S. crops

Variable	Explanation	SUR	OLS
UCTNC:	Canadian imports, U.S. cotton		
INT	Intercept	−134.141	−125.677
t-value		−1.617	−1.064
WGIXCN	Index of Canadian hourly wages	26.053	28.987
t-value		2.002	1.567
PSOYCN	Price, U.S. soybeans, deflated Canadian dollars	1.341	1.793
t-value		1.993	1.830
X7172	Indicator variable	167.689	174.914
t-value		5.602	3.446
PPLYCN	Price, polyester, deflated Canadian dollars/lb	240.555	248.162
t-value		3.998	2.859
PCTCN	Price, U.S. cotton, deflated Canadian dollars/bale	−.202	−.556
t-value		−.347	−.606
PRICN	Price, U.S. rice, deflated Canadian dollars	.265	.136
t-value		.926	.333
R-square		.623	.633
d	Durbin-Watson	2.658	2.733
t(residual)	t-value, coefficient on lagged residuals[1]	−.800	−.800
URICN:	Canadian imports, U.S. rice		
INT	Intercept	22.810	21.316
t-value		4.528	3.368
PRICN	Price, U.S. rice, deflated Canadian dollars	−.047	−.042
t-value		−3.046	−1.943
PCTCN	Price, U.S. cotton, deflated Canadian dollars	.015	.022
t-value		.505	.512
TIME	Time trend	2.158	2.144
t-value		6.352	5.118
RLPET	Real price, crude petroleum	2.414	2.309
t-value		5.585	4.215
X78	Indicator variable	6.306	7.649
t-value		3.030	2.316
R-square		.984	.985
d	Durbin-Watson	2.001	2.027
USYCN:	Canadian imports, U.S. soybeans		
INT	Intercept	618.639	604.348
t-value		7.746	5.681
PSOYCN	Price, U.S. soybeans, deflated Canadian dollars	−.969	−1.098
t-value		−2.671	−2.323
RLPET	Real price, crude petroleum	19.936	17.638
t-value		2.887	1.952
PWTCDA	Price, Canadian wheat, deflated Canadian dollars	.161	.385
t-value		.252	.392
TIME	Time trend	−15.312	−13.548
t-value		−2.776	−1.898
R-square		.563	.564
d	Durbin-Watson	2.380	2.405
t(residual)	t-value, coefficient on lagged residuals	−.887	−.887

[1]The cotton and soybean equations generated Durbin-Watson values well into the inconclusive range. For each of these two equations, I used OLS and regressed the OLS residuals against the one-period lag of the residuals, and reported the coefficient's t-value, t(residual) (see *6, 7*). Both t(residual) values suggest that evidence is insufficient at the 95-percent confidence level to reject the null hypothesis of a zero coefficient. I did not include the rice equation's t(residual) because it was nearly zero. The Durbin-Watson value fell just barely in the inconclusive range's upper end.

OLS levels by no less than 18.9 percent in all instances, by at least 20 percent in all but one instance, and by more than 29 percent in 11 of the 18 instances. Analysts involved in analyzing the North American Accord's consequences should therefore not ignore contemporaneous correlation characterizing U.S.–Canadian models of agricultural trade.

SUR/OLS Differences: Trade Elasticity Estimates

Table 3 shows estimated values of own-price elasticities of Canadian demand for U.S. cotton, rice, and soybeans (hereafter, the Canadian price elasticities). Comparable estimates from previous research were not located.

Table 2—SUR/OLS differences in standard error estimates[1]

Equation/ variable	Explanation	Difference: SUR compared with OLS
		Percent
Cotton:		
INT	Intercept	−29.8
WGIXCN	Index, Canadian hourly wages	−29.7
PSOYCN	Price, U.S. soybeans	−31.3
X7172	Indicator variable	−41.0
PPLYCN	Polyester price	−30.7
PCTCN	Price, U.S. cotton	−36.6
PRICN	Price, U.S. rice	−30.2
Rice:		
INT	Intercept	−20.4
PRICN	Price, U.S. rice	−29.6
PCTCN	Price, U.S. cotton	−29.4
TIME	Time trend	−18.9
RLPET	Price, crude oil	−21.1
X78	Indicator variable	−37.0
Soybeans:		
INT	Intercept	−24.9
PSOYCN	Price, U.S. soybeans	−23.2
RLPET	Price, crude oil	−23.6
PWTCDA	Price, Canadian wheat	−34.9
TIME	Time trend	−22.7

[1]Variables are defined in the text and table 1.

Table 3—Own-price elasticities of Canadian demands for U.S. crops

Crop	Elasticities		Differences in absolute values
	SUR-estimated model	OLS-estimated model	
			Percent
Cotton	−0.121	−0.332	−63.6
Rice	−.162	−.146	11.0
Soybeans	−.334	−.378	−11.6

SUR/OLS differences in the Canadian price elasticities fall within the 11-12 percent range for the rice and soybean relations and exceed 63 percent for the cotton equation. This article's SUR and OLS point estimates vary for each coefficient and therefore for implied trade parameters (*2*). Policy decisions are based on such point estimates, which vary across even unbiased estimators, and are not based on the unknown expected values, which are equal across unbiased estimators. Accounting for Canada's cross-crop contemporaneous correlation emerges as an important concern for researchers who analyze U.S./Canadian trade in farm products. For example, a proposed policy's cotton price reduction would imply a far smaller predicted effect on Canadian cotton demand should the analyst use the SUR-generated own-price elasticity of −0.121 rather than the OLS-generated estimate of −0.332 (table 3). One expects unbiased SUR and OLS estimates of the coefficients and resulting trade parameters (*6, 7, 8*). One may not necessarily expect, however, that correcting for the model's cross-crop contemporaneous correlation generates differences of more than 60 percent in the policy-relevant point-estimates of Canada's own-price elasticities for a crop.

When confronted with seemingly unrelated Canadian demands for U.S. crops, analysts should de-emphasize the equality of unknown expected values of a particular coefficient's unbiased SUR and OLS estimates. Analysts should rather stress how the coefficient's point-estimates differ across the two unbiased estimators.

Forecast Accuracy Beyond the Sample

I calculated the annual forecast errors and mean absolute percentage errors (MAPE's) for 1983-85 (the validation period), 3 years beyond the sample period. By following a recent study's procedure (*2*, pp. 18-19), I provide the naive model's forecasts for comparison because comparable validation results were not located. A naive prediction is the prior period's observed value. Table 4 provides the information on forecast performances.

Note that the 1983-85 validation period spanned a time of great uncertainty concerning the provisions of the then-imminent Food Security Act of 1985 (*2, 3*). This uncertainty may explain the rather large MAPE's for the estimated and naive models. Both versions of the estimated equations predicted more accurately than the naive model in two of the three cases.

Recall that SUR and OLS estimators of seemingly unrelated equations are unbiased, generating coefficient estimates with equal expected values (*2, 7, 8*).

Yet, forecasts are not made with the unknown expected coefficient values but with the coefficient point estimates, which clearly vary across even unbiased estimators (see table 1). SUR point estimates vary from OLS levels in a manner that improves the Canadian model's overall forecast accuracy in two ways. First, table 4 illustrates that accounting for Canada's cross-crop contemporaneous correlation results in a higher or worse MAPE in only one of the three modeled markets. The SUR-generated MAPE's were as good as, or better than, OLS-generated levels in two markets. Second, the SUR-generated annual forecast errors were less than OLS errors for every year, or nearly every year, for equations whose SUR MAPE's are equal to or less than the OLS MAPE's.

The results suggest that correcting for Canada's cross-market contemporaneous correlation provides forecast performances as good as, or superior to, performances of the OLS model in most markets.

Conclusions

Sample evidence suggests that the own-price elasticities of Canadian demands for cotton, rice, and soybeans are highly inelastic (see table 3). Accounting for the model's contemporaneous correlation resulted in declines from OLS levels in the standard error estimates of each coefficient, as expected from econometric theory, and enhanced the reliability and precision of policy-pertinent inference parameters. The large degree of these gains, however, is study-specific (see table 2). Researchers and policymakers should note the contemporaneous correlation's large distortions of inference parameters that are relevant to U.S.-Canadian trade in farm products. Correcting for the Canadian import model's contemporaneously correlated disturbances with SUR-generated large differences from OLS levels in the point estimates of coefficients and in certain trade parameters. The SUR version, which corrected for Canada's cross-market contemporaneous correlation, predicted as accurately as, or more accurately than, the OLS version, which ignored the problem, even though both versions' estimates were unbiased. Analysts should rely less on the property of equal but unknown expected values and should stress how greatly the policy-pertinent point estimates differ across unbiased estimators. SUR should be used, and OLS avoided, when estimating this article's seemingly unrelated equations of U.S./Canadian crop flows, even though both techniques are unbiased. Correcting for contemporaneous correlation greatly influenced both the size of the policy-relevant trade parameter estimates and the forecasts of Canadian purchases of U.S. crops.

Table 4—Forecast errors and mean absolute percentage errors (MAPE's) of forecasts, 1983-85

Crop/version	1983	1984	1985	MAPE
		Percent		
Cotton:				
SUR	−18.7	−10.2	7.1	12.0
OLS	−14.6	−5.4	11.4	10.5
Naive	−26.0	4.5	37.0	22.5
Rice:				
SUR	−12.5	−11.9	41.4	21.9
OLS	−12.6	−12.0	41.2	21.9
Naive	−8.7	0	59.7	22.8
Soybeans:				
SUR	16.7	22.4	59.7	32.9
OLS	19.0	25.6	66.7	37.1
Naive	48.1	13.9	33.0	31.7

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Two Methods for Estimating Real Structural Change in Agriculture

Robert Reining

Abstract. *The regression method of adjustment for price changes produces estimates that are close to those produced by the reclassification method, especially when the results are aggregated into three sales classes. The difference between the two methods is greatest for the smallest sales classes. Although both methods produced similar results, the regression method is faster, much less expensive, and more flexible than the reclassification method. Estimated are census farm numbers and cash receipts by farm sales class from the census of agriculture in terms of constant 1982 farm prices for 1974 and 1978 using a low-cost regression method.*

Keywords. *Agricultural structure, inflation compensation, statistical methods.*

Structural change in U.S. agriculture refers to change in the distribution of farms and cash receipts by sales class. The rapid concentration of sales on farms in the largest sales classes in recent years is a cause for concern. Accurate analysis of structural change in U.S. agriculture must be achieved before we can understand and address concerns about concentration.

A unit of analysis must be chosen from a list of imperfect options. Two common choices are acreage classes (farm size based on acres) or sale classes (farm size based on gross sales). Using acreage classes is unwise here because land is a highly heterogeneous resource. Different commodities vary greatly in output per acre, and the number of acres on a farm may have little to do with a farm's gross output or value of production. Sales classes are better for national analysis of structural change from an economic point of view.

A major problem with analyses based on sales classes is that the census data on farms according to sales classes are based on the nominal prices of commodities sold in the census year. Changes in prices between census years tend to move large numbers of farms between sales classes whether or not they actually had increases or decreases in sales. Price changes, therefore, make nominal-price data unsuitable for accurate structural analysis. Other studies adjust farm numbers by sales class to compensate for price changes (*1, 2, 3, 4*).[1] These studies show estimated farm numbers in terms of constant prices for different intervals in different base years.

I consider two methods for adjusting farms, sales, income, and acreages for price changes. One approach directly reclassifies the individual records from the census of agriculture data tapes (*11*). That approach involves adjusting gross sales of crops and livestock products from individual farms by using the national indices of prices received by farmers. Each farm is then placed into the appropriate constant-price sales class. This reclassification is useful for detailed analysis because it preserves the relationship between individual farms and their assets and attributes. However, the cost to reclassify the Census Summary Table of Farms by Sales Class for a single census year would be $7,500-$10,000 (*5*). Only 1974 and 1978 census data have been reclassified using 1982 price levels. The feasibility of processing data tapes for the 1969 census or earlier census years is unknown, and if feasible, the cost would probably be higher than for the 1974 to 1982 censuses.

An alternative to direct reclassification is a regression method that adjusts the published data on farms according to the general index of all prices received by farmers. The regression method, performed on a personal computer with standard spreadsheet software, is also more flexible than the reclassification method in that updates or changes in the reference year can be made easily, and data from the 1969 census or earlier census years can be adjusted. The regression method, however, may be less accurate than the reclassification method because it operates on data aggregated into as few as eight sales classes instead of the individual farm records. The regression method does not directly preserve data on the assets and attributes from the records of individual farms.

Method

Lin and others first used the regression method to adjust farm numbers for price changes (*2*). The regression method is applied to a cumulative distribution of farm numbers. In the cumulative distribution of farm

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[1]Italicized numbers in parentheses cite sources listed in the References at the end of this article.

numbers, the number of farms in each sales class is the sum of the number of farms in that sales class and above. Therefore, the smallest sales class in the cumulative distribution has the total number of U.S. farms while the highest sales class has only the number of farms that actually had sales at that level. The cumulative distribution of farm numbers is inversely related to the size of the sales classes and is hypothesized to be well represented by a polynomial regression equation. Equation 1 estimates two polynomial regression equations with the same functional form for each census year. For example, to compute the amount of adjustment for farm numbers in 1974, the analyst must first regress the cumulative distribution of farm numbers in 1974 on the nominal lower bounds of sales classes (the price change factor, $I_y = 1$). Then, the same distribution of farm numbers must be regressed again on a set of sales class bounds that have been shifted by a price change factor (I_y) proportional to the amount of inflation in agricultural prices between 1974 and 1982.

The two estimated distributions are then compared with each other to estimate net change due to price changes (equation 2). Subtraction of the distribution of net changes in farm numbers due to price changes from the nominal-price distribution of farms produces an estimate of the constant-price number of farms.

Estimates of sales and other attributes of the farms in each sales class are adjusted based on the assumption that these attributes can be shifted in proportion to the shift in farm numbers resulting from the adjustment method.

$$FNA_y(L) = \ell n\alpha + \sum_{n=1}^{n} \beta_n(\ell nL(I_y))^n, \qquad (1)$$

$FNA_y(L)$ = Cumulative number of farms that had sales in excess of L in a census year (y)

L = Lower bound of a census sales class in nominal prices

N = Degree of polynomial function

I_y = Deflation (inflation) adjustment factor; the ratio of the index of prices in the base year to the census year (y)

$\alpha\ \beta_n$ = Parameters of the distribution

$$\text{Net change due to price changes} = \underset{\text{[gain due to price changes]}}{(NB_n - NA_n)_y} - \underset{\text{[loss due to price changes]}}{(NB_{n+1} - NA_{n+1})_y} \qquad (2)$$

NB = Estimated number of farms in sales class n in year y prices

NA = Estimated number of farms in sales class n in year y, in base year prices

n, n+1 = Sales class, the next higher sales class

The Two Approaches Compared

The regression method produces results (table 1) for 1974 and 1978 (1982 prices) that are within a few percentage points of the farm numbers from the reclassification method for farms with sales greater than $2,500 (columns 3 and 4). Differences between the two methods probably come from index bias on the part of the regression method and underestimation of small farms by the redistribution method. Index bias may occur because cash grain sales are a larger proportion of total sales on medium farms than on small or large farms. Using a separate livestock price index and crop price index in the reclassification method may reduce index bias. However, the reclassification method probably underestimates the number of farms in the lowest sales class because only farms counted in earlier years are available for reclassification. The regression method, in contrast, tends to bring additional farms into the distribution at the lower end (less than $10,000). Yet, the regression method brings too many farms into the distribution in the smallest sales class (less than $2,500).

The regression method most closely matched the reclassification results when farms with sales of less than $2,500 were included in the nominal (input) data for the regression procedure, then truncating the adjusted distribution at $2,500 for purposes of presentation and comparison. The variance between the two methods is reduced by aggregating farms into three sales classes (subtotals in table 1), where the two distributions are essentially the same.

Sensitivity tests (not shown) demonstrated that the regression method estimation of constant-price large farm numbers was very stable. Changes in polynomial regression degree, number of sales classes, and truncation of the nominal distribution had very little effect on the estimated number of large farms.

Table 1—Comparison of constant-price (1982 dollars) farm numbers estimated using the reclassification and the regression method

Sales class	Farm numbers produced by the reclassification method		Farm numbers produced by the regression method		Regression method results as a percentage of the reclassification results	
	1974	1978	1974	1978	1974	1978
	Thousands				*Percent*	
Less than $2,500	534	407	763	406	143	100
Small, $2,500-$19,999	887	907	876	929	99	102
$2,500 to $4,999	282	291	294	315	104	108
$5,000 to $9,999	297	314	292	321	98	102
$10,000 to $19,999	308	302	290	294	94	97
Medium, $20,000-$99,999	680	673	699	679	103	101
$20,000 to $39,999	313	297	325	296	104	100
$40,000 to $99,999	367	376	375	383	102	102
Large, $100,000 and over	212	269	213	266	100	99
$100,000 to $499,999	195	246	197	244	101	99
$500,000 and over	17	23	16	23	96	99
All farms with sales greater than $2,500	1,779	1,849	1,788	1,874	100	101

Estimation of Constant-Price Sales by Sales Class

Estimates of constant price farm numbers is only part of structural change. Estimates of the constant-price distribution of sales, income, acreage, and other attributes are of interest to researchers and policymakers. Constant price sales, income, and acreage by sales class can be obtained directly through the reclassification method. I obtained constant-price sales, income, acreage and other attributes using the distribution of constant price farm numbers from the regression method. I shifted attributes in proportion to the shifts in estimated farm numbers. This results in attributes, such as sales, which sum to a value that usually approaches or equals the nominal sum inflated (deflated) to a constant-price level. Additional inflation (deflation) by means of a uniform factor may be necessary to bring the total sales or other attribute to a sum that equals the published estimates of total sales or other attributes.

Shifting sales or other attributes in proportion to shifts in estimated farm numbers is mathematically equivalent to an assumption that farms with average sales are the ones that shift between classes. The farms that move between classes are not, realistically, the same size as the average farm. The shifts in sales due to price changes may be more or less than the correct adjustment. In other words, a redistribution bias may occur.

I assessed the extent of the potential redistribution bias by comparing two sets of cash receipts that have been shifted in proportion to adjusted farm numbers. I shifted one set of cash receipts in proportion to the shifts in farm numbers resulting from the reclassification method. I shifted the other set of cash receipts in proportion to the shifts in farm numbers resulting from the regression method (table 2). In comparing the two sets of estimated constant price cash receipts, I uniformly deflated the cash receipts that were shifted by the regression method so that the two totals were the same. Not much redistribution bias occurred for farms with sales in excess of $5,000 since the two sets of cash receipts were very close. Aggregating the redistributed sales from the seven-class set to a three-class set further reduced redistribution bias, which canceled out the effect of a systematic set of differences between the estimates.

Fine-Tuning the Regression Method

The regression method helps adjust farm numbers according to prices in the most recent census year or a prior census year. Choosing an early year as the base generally results in a deflation of farm numbers in most sales classes and a decrease in the total number of farms. Choosing the most recent census year (1982) inflates the number of farms in earlier censuses. Mathematical deflation and inflation of total farm numbers resulting from the regression method is directly analogous to what occurs when the census

Table 2—Comparison of cash receipts redistributed by the reclassification method and the regression method

Sales class	Cash receipts redistributed using the reclassification method		Cash receipts redistributed using results of the regression method		Columns 1 and 2 as a percentage of columns 3 and 4	
	1974	1978	1974	1978	1974	1978
	Million dollars				*Percent*	
Less than $2,500	569	475	987	635	58	75
Small, $2,500-$19,999	7,033	6,996	7,036	7,277	100	96
$2,500 to $4,999	902	968	1,161	1,134	78	85
$5,000 to $9,999	1,989	2,068	2,008	2,194	99	94
$10,000 to $19,999	4,142	3,960	3,867	3,949	107	100
Medium, $20,000-$99,999	29,493	29,907	29,355	30,179	100	99
$20,000 to $39,999	8,392	7,818	8,359	7,916	100	99
$40,000 to $99,999	21,101	22,089	20,996	22,263	100	99
Large, $100,000 and over	55,296	75,751	55,431	75,199	100	101
$100,000 to $499,999	32,025	42,145	31,946	41,587	100	101
$500,000 and over	23,271	33,606	23,485	33,611	99	100
Total receipts for farms with sales greater than $2,500	91,822	112,654	91,822	112,655	100	100

[1]For purposes of comparison, the redistributed cash receipts on this table have been uniformly deflated to sum to the total cash receipts estimated by Ahearn (*1*). The nominal cash receipts data used in the regression method are for 1975, published in *Economic Indicators of the Farm Sector* series, deflated to equal the sum of the 1974 cash receipts.

farm definition's lower sales limit is changed up or down. For example, reducing the sales limit includes a large number of additional places with very small sales or sales potential. The census farm definition in fact has been including more farms with lower sales because the official definition is set in terms of sales in nominal prices.

Although the application of the regression method is essentially the same regardless of the base year, the effect of the method is not symmetrical. Estimation errors are probably more prevalent when the number of farms is inflated relative to the nominal-price distribution. For instance, using the current year as the base year tends to bring large numbers of farms into the distribution at the lower bound. The adjusted number of farms in the smallest sales class (farms with less than $2,500 in sales) tends, therefore, to be substantially overestimated. Inclusion of farms with sales of less than $2,500 is generally problematic regardless of the base year. Each census contains large numbers of farms with sales of less than $1,000 that are included in the official totals on the assumption that they could have had sales of more than $1,000.

The adjustment process, however, should be inherently more stable and accurate when farm numbers are being deflated. The adjustment process tends to push farms into the smallest sales classes and out of the adjusted distribution. The uncertainty about farm numbers in the small nominal-price sales classes and the nonmonotonic distribution of farms at and below the definitional boundary therefore becomes much less important. Variability in enumeration can be substantially reduced by truncating the nominal-price data set at $2,500. But, estimates of farm numbers from the reclassification method with an earlier base year are unavailable for comparison with the regression method estimates.

Both adjustment methods have inherent limits. Both methods are only approximate because the indices used are weighted averages of diverse sets of agricultural commodities, while the proportion of sales from different products is not constant across all sales classes, and commodity prices have changed at different rates. The decomposition of sales into sales of crops and livestock in the reclassification method eliminates the largest source of index bias. The index bias is generally insignificant when the change during 1969-82 is considered because price changes have tended to equalize between commodity groups. The largest potential bias exists for the regression method for the 1969-74 interval when cash grain prices increased by about 30 percent more than the index of prices received by farmers. Medium farms received about 40 percent of their sales from cash grains compared with 24 percent for large farms.

Conclusions

The regression method of adjustment for price changes produces estimates that are close to those produced by the reclassification method, especially when the results are aggregated into three sales classes. The difference between the two methods is greatest for the smallest sales classes although both methods produced similar results. However, the regression method is faster, much less expensive, and more flexible than the reclassification method.

The accuracy of estimation of farms in the smallest sales classes is probably higher when the regression method deflates farm numbers (for example, when using an earlier base year) rather than inflates farm numbers. Results from the reclassification method, however, are not available from earlier base years for comparison purposes.

Using the estimated constant price farm numbers as a basis for shifting sales of farms appears to be accurate. Similar shifts of other attributes of farms in sales classes may be sufficiently accurate for analytical purposes.

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Book Reviews

Whole Is Less than the Parts

Economic Efficiency in Agricultural and Food Marketing. Edited by Richard L. Kilmer and Walter J. Armbruster. Ames: Iowa State University Press, 1987, 315 pages, $24.95.

Reviewed by Gerald Schluter

This book leaves its readers wanting more. Not wanting more because they vibrate with intellectual stimulation or visions of new paths being carved through new frontiers of knowledge. Wanting more because of being misled and finding themselves wandering in an uncharted wilderness rather than exploring new frontiers. The book compiles symposium papers and discussions presented by an impressive list of 28 leading agricultural economists from the United States and Israel at a symposium held in 1985. The papers addressed the measuring and monitoring of efficiency in the agricultural and food marketing system. Yet, despite the credentials of its participants, the sum of the resulting proceedings is less than the parts. To borrow the technical terminology of the conference: the result is not a point on the profession's production possibility frontier but rather an inefficient use of the participants' capabilities.

As I searched for possible reasons for these unfulfilled expectations, I formed a list of "what might have beens." What if the conference organizers had circulated the lead article by Rausser, Perloff, and Zusman, "The Food Marketing System: The Relevance of Economic Efficiency Measures," before other participants started their papers? Would the others then have had a clearer idea of what was being measured and how? Would that have lessened the apparent disorganization where individual authors seem to have their own version of the dimensions of the food system and appropriate measures of efficiency?

What if a comment by William Tomek made in discussing Richard Kilmer's paper had served as a slogan or model for the conference: "It is too much to ask any single paper to deal with the entire puzzle, but the big picture must be kept in mind as we deal with the pieces"? That would have eliminated half of the 16 chapters. But, that would have been unfortunate. Some of the better discussions were the weaker, less focused articles. The just-quoted Tomek discussion fits this category.

What if the organizers and participants agreed to an appropriate measure of output of the food marketing system before they debated appropriate efficiency measures and conceptual bases for these measures? Although the book is billed as a conference book exploring economic efficiency in agricultural and food marketing, one searches in vain for a systemwide measure of output. The reader cannot find a discussion of the obvious measures: either total domestic food cost/total resources devoted for the whole food system, or total domestic food costs–farm value/total processing and distribution resources for the food marketing system. Shortly after this conference, USDA's Economic Research Service began regularly publishing an annual estimate of direct and indirect labor commitment to the food system in the *Food Marketing Review.* These estimates have been favorably received. Yet, the conference book contains not a single reference to the usefulness or the need for this type of measure and its role in efficiency analysis.

What if the editors and publishers had considered the readers' needs and interests in editing and preparing the book for publication? The decision to give readers an index was apparently made at the last minute because it is tucked into a pocket insert in the back cover. Obvious and nonsensical typos sprinkle the text, for example "a policy is a social welfare improvement if and only if $(CV_j > -CV_j)$ for all j and CV is the compensating variation measure of gains and losses" (p. 84). This strange tautological statement of Ng's quasi-Pareto criterion intrigued me. I went to the cited reference to see if the statement came from bad editing or bad writing. I still don't know. The reader will look in vain for a similar statement in the cited Ng reference. Standardization in citations is weak. For example, the National Commission on Food Marketing, a group that published its final report in 1966, is referred to at least three times in three different ways. Yet readers unfamiliar with the group who referred to the "afterthought" index would find that the only reference to this commission in the index was a citation that doesn't give a source. Evidently, the reader is supposed to be familiar with it.

Should JAER readers ignore this book? No. While it fails to deliver what's intended and the editors and publishers contribute to a reader-unfriendly book, it

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contains some strong individual papers. James MacDonald, for example, in his discussion of economics of scope and contestability theory, and Nancy Bockstael, in her discussion of grading and minimum quality standards, both handle their special area well in following the Tomek "model" of keeping their eye on the big picture while dealing with the pieces. Other notable contributions include the three subsection summaries by Ben French (economic efficiency), Ron Ward (concepts for evaluating economic efficiency), and Richard Heifner (economic efficiency, public programs, private strategies).

In addressing timely and appropriate topics, the book drew on an impressive list of contributors. But by allowing many unfocused papers, it missed a great opportunity. It makes disappointingly little progress toward even defining the issues. How or why would you want to measure efficiency if you didn't know or care about the level or nature of your output?

The papers include: Section I—Economic Efficiency: (1) "The Food Marketing System: The Relevance of Economic Efficiency Measures" by Gordon C. Rausser, Jeffrey M. Perloff, and Pinhas Zusman; a discussion by George W. Ladd, (2) "Economic Efficiency and Welfare Measurement in a Dynamic, Uncertain, Multimarket World" by Richard E. Just; a discussion by Rulon Pope, (3) "The Science and Art of Efficiency Analysis: The Role of Other Performance Criteria" by J. Walter Milon; a discussion by Emerson M. Babb, (4) "Does the Concept of Economic Efficiency Meet the Standards for Truth in Labeling When Used as a Norm in Policy Analysis?" by James D. Shaffer, and (5) "Comments on Efficiency" by Ben C. French.

Section II—Concepts For Evaluating Economic Efficiency: (6) "The Economic Efficiency of Alternative Forms of Business Enterprise" by Ronald W. Cotterill; a discussion by Lee F. Schrader, (7) "The Economic Efficiency of Alternative Exchange Mechanisms" by Richard L. Kilmer; a discussion by William G. Tomek, (8) "Economies of Scope, Contestability Theory, and Economic Efficiency" by James MacDonald; a discussion by Timothy G. Taylor, (9) "Economic Efficiency and Market Information" by Frances Antonovitz and Terry Roe; a discussion by Dennis R. Henderson, and (10) "Comments on Concepts for Evaluating Economic Efficiency" by Ronald W. Ward.

Section III—Economic Efficiency, Public Programs, Private Activities: (11) "Economic Efficiency and Marketing Orders" by Edward V. Jesse; a discussion by Mary C. Kenney, (12) "Economic Efficiency Issues of Grading and Minimum Quality Standards" by Nancy E. Bockstael; a discussion by John P. Nichols, (13) "Futures Markets and Intertemporal Commodity Pricing" by Anne E. Peck; a discussion by Sarahelen Thompson, (14) "Efficiency in Commodity Storage" by Bruce Gardner; a discussion by Jerry A. Sharples, and (15) "Comments on Economic Efficiency, Public Programs, and Private Strategies" by Richard G. Heifner.

Section IV—Summary and Research Directions: (16) "Economic Efficiency and Future Research" by Richard L. Kilmer and Walter J. Armbruster.

Agricultural Policy: Can It Cope With a Changing World?

U.S. Agriculture in a Global Setting: An Agenda for the Future. Edited by M. Ann Tutwiler. Washington, DC: Resources for the Future, 1986, 234 pages, $20.00.

Reviewed by Marc O. Ribaudo

Traditional farm policies cannot cope with the rapidly changing environment in which U.S. agricultural policy operates, according to the editor of *U.S. Agriculture in a Global Setting: An Agenda for the Future.* The contributors present convincing evidence that fundamental changes in farm policy and legislation are necessary. The book does an excellent job of identifying and discussing the forces that affect the goals and rationales for agricultural policy.

The title is a bit misleading. It implies a discussion of the changing role of the United States in the international economy. However, the book goes beyond trade issues by examining two forces that affect agricultural policy: forces within the agricultural sector itself and forces outside the domestic farm sector. Many farms have become larger, more heavily capitalized, and more specialized. There have been large gains in production technologies, and the number of farms has declined. The decline in political power of the traditional agricultural lobby has led to many commodity-based interest groups, affecting how agricultural policy is created.

U.S. agriculture's position in the global economy has become increasingly vulnerable to changes in domestic and international trade policies and patterns. Demand for agricultural products has changed in response to changing demographics and increasing awareness of nutrition and food safety. Rural economies' diversification, with less reliance on agriculture, has reduced the economic effect of agricultural policy on rural areas.

Resource adequacy and environmental quality have become major issues for farmers. There is growing concern over the availability of land and water to produce adequate levels of food and fiber economically in the future. There is also concern over the environmental effects of the use of pesticides and nutrients, soil erosion, and loss of habitat. To complete the list, the authors even included changes in global climate as a potential effect on agricultural policy. This is a growing concern in agricultural circles and may become one of the major issues in the near future.

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The forces affecting agriculture are placed in the context of traditional agricultural policies, which, the authors argue, have been essentially unchanged since their inception in the 1930's. An excellent presentation of the historical background of the current policy environment, in chapters by Allen and Elliot, Thompson, and Browne, provide the economic, philosophical, and political forces that shaped traditional agricultural policy. Traditional policies were designed mainly to increase and maintain farm income (and the economic health of rural areas), to prevent large swings in agricultural prices, and to preserve the traditional family farm.

However, the authors maintain that traditional commodity-based policy is unable to cope with the changes outlined above. They present evidence that those policies have not protected the agricultural sector from the effects of external forces on farm income, and that they have encouraged environmental degradation and misuse of natural resources and hampered our competitiveness in the world economy. Attempting to deal with trade, environment, and other new issues facing agriculture through traditional programs will lead only to failure, probably expensive failure.

The papers include: (1) "The Current Debate and Economic Rationale for U.S. Agricultural Policy" by Kristen Allen and Barbara J. Elliott, (2) "The Philosophical Rationale for U.S. Agricultural Policy" by Paul B. Thompson, (3) "An Interdependent and Fragile Global Economy" by M. Ann Tutwiler and Barbara J. Elliott, (4) "Brighter Prospects for Agricultural Trade" by Fred H. Sanderson and Rekha Mehra, (5) "International Agricultural Negotiations: The United States and European Community Square Off" by Michel Petit, George E. Rossmiller, and M. Ann Tutwiler," (6) "Consumer Demands: A Balancing Act" by Carol S. Kramer, (7) "The Changing Face of Rural America" by John J. Kornacki, (8) "The Fragmented and Meandering Politics of Agriculture" by William P. Browne, (9) "Agricultural Information and Technology: A Continuum of Change" by A. Barry Carr, (10) "Land and Water: Will There Be Enough for Agriculture?" by Pierre R. Crosson, (11) "Some Rays of Hope for Agriculture and the Environment" by Tim T. Phipps, (12) "Global Climate Change Holds Problems and Uncertainties for Agriculture" by Norman J. Rosenberg, and (13) "An Agenda for the Future" by M. Ann Tutwiler and A. Barry Carr.

The forces acting on agriculture are redirecting agricultural policy away from traditional commodity tools and toward a more broadly defined policy. The authors do not make specific policy recommendations, but present a general agenda for change. The major recommendations include:

- Reassess the goals of agricultural policy, and determine which problems can best be dealt with through agricultural policy.

- Develop programs that minimize market distortions.

- Emphasize education to enable farmers and other rural residents to cope with changes.

- Focus on encouraging growth in less developed countries, thereby creating markets for U.S. products.

- Integrate environmental and agricultural policy.

One set of recommendations that could have been made involves the institutional framework within which policy is made. Proliferation of special interest groups and subcommittees involved with agricultural policy was justifiably cited by Browne as a major problem in creating flexible, consistent policy. Major changes in the relationship between Congress, the President, and special interest groups would have to occur before a new agenda for agricultural policy could be enacted. Whether such a change will be made is highly questionable.

Multifactor Productivity Measurement: A Wild Goose Chase?

Productivity and Value: The Political Economy of Measuring Progress. *By Folke Dovring. New York: Praeger, 1987, 194 pages, $35.00.*

Reviewed by James H. Hauver

Folke Dovring challenges conventional economic theory in this important new book by arguing that multifactor productivity measurement is conceptually invalid due to insurmountable aggregation and index number problems. Instead, he proposes a disaggregated approach, relying on accumulated single-factor partial productivity measures.

Dovring fails. He ignores important developments in index number theory, which mitigate the problems he describes. He is fundamentally mistaken in his evaluation of chain-link indexes (such as Divisia indexes). He exaggerates the apparent weaknesses of neoclassical production theory, minimizing, for instance, the significance of price adjustment in the working of the economy. He treats labor and capital inconsistently by exaggerating labor's homogeneity. And, he offers no acceptable alternative to multifactor productivity indexes.

Dovring's critique of multifactor productivity measurement is founded on a rejection of neoclassical production theory. Dovring places particular stress on the impossibility of aggregating capital. Capital goods, he argues, are too heterogeneous in purpose or structure, presenting unique problems due to their duration, obsolescence, and variation in lifetime. They have no common quantitative unit analogous to, say, the acre or Btu.

Aggregation problems provide the rationale for Dovring's rejection of multifactor productivity measurement. The measurement of multifactor productivity assumes that both inputs and outputs can be measured and aggregated in a manner that makes them acceptable proxies for physical quantities. Because no common physical unit of measurement exists, aggregation can take place only through the use of constant prices. Dovring argues, however, that the use of constant prices depends upon two false assumptions: that price weights can be used to give meaningful comparisons over time, and that constant prices represent efficient market outcomes. Nor can chain-link indexes provide a basis for aggregating either outputs or inputs.

Dovring argues that multifactor productivity measures are not objective gauges of real productivity gains but reflections of underlying social structure. To escape from institutionally determined class laws, Dovring argues, we must abandon multifactor productivity measurement, with its dependence on market values. Factors must instead be specified in physical terms, which can be done only on a factor-specific basis.

To avoid methodological nihilism, Dovring proposes a return to partial productivity indexes but on a revised basis. Recognizing the limitations of simple partial productivity indexes, Dovring proposes the use of accumulated single-factor productivities for energy, land, and, especially, labor. An accumulated labor productivity index includes both labor used directly in production, and indirect labor embodied in other factors of production. Dovring rejects quality adjustment of labor.

Capital goods must be treated differently. They are heterogeneous and have no common physical unit in common. Nonetheless, capital goods incorporate land, labor, and energy. An economist can calculate how much of these resources have gone into the accumulation of the capital stock. The contribution of design changes (a purely qualitative concept) to overall efficiency improvements can be indirectly inferred from the accumulated quantities of land, labor, and energy. Ultimately, one can obtain an estimate of the capital coefficient without aggregating capital at all.

The conceptual and practical difficulties facing productivity measurement do not justify abandoning the whole enterprise of multifactor productivity research. Real world empirical research will never satisfy all the strictures of pure aggregation theory, but the gap has narrowed over the past 20 years. Researchers have found that some of the most restrictive assumptions used in model building and estimation can be relaxed. Weak separability, for instance, is not required for the estimation of a Tornqvist index of productivity growth. The assumption of homotheticity can also be relaxed. Duality theory, flexible functional forms, and hedonic indexes are theoretical tools that are now expanding the scope and power of productivity research. Economists have a better understanding of the assumptions implicit in their models and the data required for implementing new approaches.

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Economists can initiate improvements in productivity measurement. First, the new fields of duality theory and hedonic indexes appear to offer promising avenues for future productivity research. Duality theory and flexible functional forms have enabled economists to relax many of the stringent assumptions previously required for empirical estimation in productivity studies. Hedonic models express goods in terms of their characteristics rather than the inputs used to produce them, providing a better representation of production technology. Second, capital theory and measurement are probably the weakest areas of productivity measurement. Economists need more accurate and timely estimates of stocks, improved depreciation estimates, and some valid measure of lifetimes of different types of capital stocks. Quality changes in inputs in general are important in understanding the sources of growth.

The U.S. Department of Agriculture (USDA) is already improving its existing productivity measures through the introduction of national Tornqvist indexes of outputs, inputs, and productivity. The new indexes will appear in the next annual *Economic Indicators of the Farm Sector: Production and Efficiency Statistics* in 1989. Along with the shift toward Tornqvist indexes, USDA is making substantial improvements in its estimates of the individual input components.

Dovring fails to offer a credible alternative to multifactor productivity measurement. He exaggerates the homogeneity of land, energy, and especially labor, in contrast to capital. How can the labor of a starting level professional be considered homogeneous with that of someone with 15 years experience in the same profession? Accumulated single-factor measures are not comprehensive, as Dovring attests. They ignore the contributions of other factors not accounted for in indirect labor. Because capital intensities can vary across economic sectors, the direction of the resulting bias is unclear. Dovring relies on input-output tables to derive technical coefficients for computing indirect labor and energy. Technical coefficients are fixed and can be outdated. Fixed coefficients fix productivity relationships and constrain substitution possibilities. Consequently, Dovring's proposals represent a retreat in productivity research.

Dovring's study of productivity is admirable in some ways. The book is sweeping in scope, and despite its moderate size, covers many productivity issues in surprising depth. Dovring explores the issues raised in analyzing productivity in land, labor, capital, and combinations of inputs. He presents a lucid analysis, remarkably free of technical jargon unless absolutely necessary. Dovring's analysis is nonmathematical, suitable for a general reader and successfully avoids slipping into superficiality. Dovring's work remains cogent, substantial, and penetrating. The book is well worth reading, even by the specialist in productivity studies.

The heart of Dovring's argument, nonetheless, is an attack on multifactor productivity measurement. His arguments fail in destroying the foundations of multifactor productivity research. His proposed alternatives offer no attractive substitute.

The Resources of Agricultural Economics—The Profession's Assessment

Agricultural and Rural Areas Approaching the Twenty-first Century. Edited by R.J. Hildreth, Kathryn L. Lipton, Kenneth C. Clayton, and Carl O'Connor. Ames: Iowa State University Press, 1988, 565 pages, $14.95.

Reviewed by Harold F. Breimyer

In 1985, the American Agricultural Economics Association (AAEA) held its annual meeting on the Iowa State University campus where the organization had been founded 75 years earlier. It commemorated its 75-year history in a forward-looking manner. Noting that the next anniversary would fall in the next century, the association took stock of the profession's resources and assessed their applicability to the future. The project led to publication of the book reviewed here.

The 57 eminent agricultural economists who stayed on campus responded to the charge, as stated by R.J. Hildreth, "to identify the challenges facing agriculture and rural areas in the twenty-first century" (p. ix).

They gathered, according to Hildreth, "to redefine the issues we face as a profession and to point the way to a sharper, more relevant set of priority issues . . ." (p. ix). What was surprising, and perhaps least in keeping with the 75-year AAEA history, was that priority issues were to be defined in order to qualify for "competitive and special grants . . ." (p. x). A second purpose, Hildreth says, was to assist individual agricultural economists in allocating their talents to "topics that are challenging as well as in the national interest" (p. x).

The book is a work of high scholarship that reports the contemporary scene well but defaults on futurism. The book fails to give the imaginative, introspective glance into the next century that it promises.

The 12 principal chapters, each followed by two or three brief discussion papers, cover topics ranging from technology to institutions to quantitative techniques to world agriculture. The chapters nevertheless divide rather sharply between those that constitute an inventory of both factual knowledge and techniques and those that are essentially essays of interpretation, which are the more subjective and provocative because the authors address not only how things are but how they ought to be.

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Among chapters of the first type are two by Glenn Johnson and Stanley Johnson. Both are remarkably comprehensive. Glenn's covers technological innovations and Stanley's, quantitative techniques. One discussant, David Bessler, says that Stanley Johnson surveys "virtually all interesting (useful) techniques in use today," plus offering "a thoughtful view of future developments" (p. 199). Bessler hasn't much to add.

The stock-taking articles are scholarly, will fend off rebuke, and can be useful as assignments for graduate students. At the same time, they are as exciting as a report of a ladies' sewing bee.

Authors from various fields contribute to the interpretive chapters. Several themes are pervasive. Least surprising is the recurring international focus. Hildreth mandates it, Mellor and two discussants pursue it, Schuh and Orden convert their macroeconomics topic into it, and few authors fail to mention it. Sometimes the adjective "new" is attached, oblivious to the historical fact that U.S. agriculture has always been international. The international dimension is not new, just inscrutable.

The Schuh-Orden restatement of what has become Schuh's signature piece on monetary exchange brought retorts from Rausser and Boehlje, who say the authors do not address their assigned macroeconomics topic. Rausser, although agreeing "that macroeconomics should be studied and that it matters" (p. 384), objects to the Schuh-Orden empirical analysis, which he calls "most peculiar" (p. 392).

The more cutting ripostes in the book, however, relate to themes that define the scope of agricultural economics. Should the scope be narrow or broad? Several authors who favor the broad approach are concerned for tests of correctness and applicability of economic knowledge. Welfare economics gets frequent mention. Tweeten calls for "the professional equivalent of placing a man on the moon—specifying a social welfare function" (p. 142). Tweeten dallies with the old distinction between normative and positive, then strikes comparisons between prescriptive and descriptive. His discussants, stricter interpretationists, disagree vigorously. Bender sees little difference between Tweeten's prescriptive economics and Milton Friedman's characterization of positive economics (p. 146). Bullock is "surprised" by Tweeten's emphasis on a social welfare function (p. 149). Paarlberg has "grave misgivings about the ability of economists to prescribe agricultural policy actions or to estimate a social welfare function" (p. 155).

Tweeten outlasts his critics. Welfare criteria, and the political-economy focus that predates economics, pop up frequently in the big volume. De Janvry, charged to discuss Mellor's paper on world agriculture, takes up the Tweeten cudgel: "Since the role of government is so important in agriculture, agricultural economics [economists] should not miss the opportunity of being at the frontier of this important field" (pp. 173-74).

Daniel Bromley stirs debate as he declares "resource problems" to be "entitlement problems" to which "welfare economics provides us with no unambiguous answer. . . ." (p. 208). Further, welfare economics is now on "the threshold of a third phase . . . that will be primarily concerned with 'situational conflicts' as opposed to general efficiency phenomena." Also, "confrontation and conflict will predominate . . . and . . . the essence of that conflict will be over presumed (or actual) rights and duties on the part of those bearing joint costs" (p. 226).

Alan Randall, a discussant, straddles the role of devil's advocate and being the devil himself, then protests that "75 or 80 percent of practicing natural resource economists . . . would not identify themselves with the institutionalist land economics tradition" (pp. 230-31). Readers who ponder what Barkley, Alter, Powers, and Raup say in a later chapter devoted to institutions, and note how often various writers address institutional design, may wonder whether Randall is sketching a meaningful distinction or nitpicking. Too many of the discussants quibble about matters that have little bearing on agriculture and rural areas of the 21st century.

Technology shows up frequently in the context of an alleged new information age with its speeded communication. Glenn Johnson asks if private and public electronic controls will substitute for the "allocative functions of the market mechanism" (p. 84). Knutson, who never equivocates, makes the cogent suggestion that insofar as research findings in new "biotechnology and information technology" will be "patented and sold . . ." (p. 117), publicly sponsored research may come to an end. He stops short of asking whether knowledge is no longer to be a free good.

In a different vein, risk and uncertainty are mentioned repeatedly. John Ikerd counsels that "farmers must learn to cope . . ." (p. 539), as though they have not done so from infancy. Prior knowledge alone, without accompanying wherewithal, will not reduce risk very much.

Not all the highlights in the book can be noted here. Pope's listing of new fields of emphasis, Deaton and Weber on how agricultural economics once came close to being called rural economics, and Bonnen on the data base are worth reading. When Deaton and Weber treat rural economies "in a neomercantilist age" (p. 409), Hady objects. He views "increasing mercantilism as a problem of the 1990s" and hopes that "saner heads will prevail by the early 2000s" (p. 447).

Bonnen deplores "the profession's growing flight from the empirical" (p. 452). Crosson hesitates but decides to agree. George Hoffman calls "Bonnen's concern . . . well founded" and the problem "a threat to the long-term viability of the agricultural economics profession" (p. 496).

The 57 authors tell where agricultural economics now stands and do a little extrapolating. But not one futurist is to be found among them. As an exercise in clairvoyance, promised by its title, the book is a dud. There's no mention of how depletion of the world's petroleum reserves will affect the next century. Eisgruber notes that King and Sonka, when writing about management problems, omit mention of environmental management (p. 299). Mellor addresses world poverty, but neither he nor any of his discussants suggests that runaway birth rates could convulse much of the world.

The agricultural economists who met at Iowa State University and were charged with itemizing the profession's current resources for the purpose of preparing for the future overfulfilled the first part of the assignment and fell short on the second. If members of the profession aspire to be futurists, they must take a new look at both themselves and the signs, however indistinct, of what lies ahead for the Nation and its agriculture.

Financial Stress on the Farm

The Farm Financial Crisis: Socioeconomic Dimensions and Implications for Producers and Rural Areas. Edited by Steve H. Murdock and F. Larry Leistritz. Boulder, CO: Westview Press, 1987, 186 pages, $18.95.

Reviewed by Jeff Tripaldi and Joel Schor

Farmers have never been able to control all the variables necessary to grow and market their crop profitably. Despite success in increasing production, farmers have become more, rather than less, dependent on external factors. The assistance of agricultural economics and science to the farmer is valuable, provided that prognostication is accurate and production is adjusted in line with domestic and foreign demand. In the early 1980's, the forecasts were inaccurate, and as a result, the recent farm financial crisis caught the farm sector by surprise. The editors discuss the origins of the crisis, provide an important definition of the term financial stress, and identify both primary and secondary effects in rural America.

Although the Great Depression too began in rural America before spreading to the rest of the economy, the recent economic distress on farms is not analagous. The editors document the uneven nature of that distress, how it fell disproportionately on the middle class ($40,000–$150,000 sales), innovative, and youthful producers (an entire profile is provided). That the entire farm sector was not distressed, however, provides little comfort for a number of reasons: (1) those distressed were among the most innovative, creative, leadership elements within rural areas; (2) their number was high, conservatively computed by the editors at 213,500; (3) the secondary and tertiary effects of such a displacement may surpass 1 million workers; and (4) this may erode substantially the revenue base of rural America.

On the other hand, overall production of food and fiber is not likely to be reduced by the crisis, and consumer prices for food are not likely to increase much. Nevertheless, the authors of the papers in this book conclude that from a fourth to a third of the population in agriculturally dependent counties will leave over the next few years. Because these migrants will constitute the vigorous leadership and the older and less active will remain, the future shape of agriculture is very much an issue.

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Crucial to their discussion is the debt/asset ratio, which at 40 percent or above becomes a determinant in predicting stress. In the early chapters, the historical causes of the crisis are laid out briefly: the liberal credit policies of the 1970's and 1980's, the contraction of markets in the early 1980's (which went unnoticed by most economists), the increasing farm debt, and the decline of land values (upon which the level of farm equity/credit is based at the bank), all accompanied by declines in farm income. The recessionary period from 1979-82 was the most severe since the Depression, the editors assert, and was accompanied by record high interest rates. To them, the first event in this chain of causation was the decision by the Federal Reserve Board in October 1979 to limit the supply of credit to limit inflation. The effect of tight money, they contend, was compounded by the fiscal policy embodied in the Economic Recovery Act of 1981. Tax cuts of such a magnitude virtually ensured massive deficits. Thus, the farm sector would experience a large reduction in credit.

As defined by the authors, farm financial stress contains a social/psychological component—a feeling of helplessness, frustration, anger, and despair—from knowing that one can no longer meet one's cash-flow commitments. The editors derived from ERS publications and private surveys much of the data compiled to measure the degrees and forms of such stress. Although the material is by no means complete, it is probably the best available at the moment and is clearly presented in graphs, charts, figures, and illustrations.

In their concluding chapter, the editors suggest policy options, though these are not spelled out in detail: direct financial assistance programs aimed at reducing producers' overall levels of debt; programs aimed at assisting producers in renegotiating loans and altering payment schedules; assistance to producers in improving their management and decisionmaking skills; assistance to producers in retraining to obtain nonfarm employment and assistance in relocating to alternative job sites; the provision of mental health and other human services (p. 172).

Written before the drought of 1988 and before the impact of disaster aid to farmers and therefore unable to assess the effect upon the existing crisis, the work nevertheless suggests the crisis will continue. One impact not discussed is the effect the changing structure of agriculture will have upon the quality of the

food supply. Will the production methods employed by the large corporate farmers, who will replace the middle class in the editors' view, result in the same quality of food and fiber as that of the more traditional methods of the family farmers? Clearly, the urban consumer has a stake in the outcome but is unlikely to read such a work.

While this book covers well the area delineated, and is important to the overall understanding of farm stress, another volume dealing with the likely impacts for urban areas and other sectors of the economy appears necessary. While recent data tend to reduce the impact of the farm crisis, it is still too early to predict the effects with precision.

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